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Reviews the literature since the issuance of Vol. XXI, No. 5, December 1951, and Vol. XXIV, No. 5, December 1954.

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INTRODUCTION

This issue more nearly resembles the parallel issue in the 1951 cycle than its predecessor of 1954. Whereas the latter was devoted to a review of statistical developments, the former attempted to review developments in research methodology generally. However, we have set ourselves an additional task. Specifically this issue attempts to:

1. Take note of the significant literature published in the intervening period since the last REVIEW coverage of certain familiar areas of research methodology
2. Introduce new topics of immediate or potential importance to those engaged in educational research work.

This latter goal is exemplified in the issue's first chapter. This discussion of the philosophy of science was included to point up the importance of the field's developments for the model building and theorizing upon which the future progress of much of educational research so heavily depends. Another example is Chapter II on cross-cultural methods. It spotlights a method which some workers believe holds the key to an interdisciplinary approach to some of education's important problems, and it emphasizes the "science" aspect of the "social sciences." Still another example is Chapter IX. The electronic calculator has become more than a slave to do routine problems. It opens up new avenues of research and makes practical the use of research methods which were formerly given but fleeting attention because they were too complex and cumbersome.

The original design of the issue called for a chapter to continue the excellent discussion of decision theory in the December 1954 issue of the REVIEW. While several authors expressed considerable faith that in the future exciting and valuable developments would stem from this area of investigation, they agreed that it was too early to re-examine this field.

The problem of facilitating the use of research results has become almost as crucial as that of developing new research findings. Chapter X discusses one method of attacking this problem, action research, and points to an important weakness in conventional research methodology to which some attention should be given.

The organization of this issue roughly corresponds to the time sequence in which questions might present themselves in the research process: (a) a philosophical consideration of the research process and the function of models (Chapter I); (b) the consideration of the various research methods, the choice of technics, and the design of the experiment (Chapter II thru the Status Studies of Chapter IV); (c) the consideration of various research tools to develop problem treatment (the "sampling" part of Chapter IV thru Chapter VIII); (d) data processing (Chapter IX); and (e) use of results (Chapter X).

The increased scope of this issue has forced even greater emphasis on the REVIEW's traditional policy of being selective rather than comprehen-

sive in its bibliographic coverage. While this is specifically noted in the introductions of certain chapters, it is true of nearly all of them. In this field of the REVIEW's cycle increasing pressure for selection comes both from the intensive development of existing technics and from the continued addition of new ones. Consideration should be given in forthcoming numbers to attempting more regular and intensive coverage of a limited field on alternate cycles (e.g., the December 1954 REVIEW) and/or depending more heavily on other issues in the cycle to help complete coverage.

To some extent this latter policy is already in effect. While the unity of educational knowledge is such that probably no REVIEW issue finds the borders covered solely within its pages, this area is especially fortunate in that help is obtained from several parts of the cycle. For instance, test methodology and measurement theory are covered in more detail in the REVIEW of February 1956. Similarly the issue, "Human Relations and Programs of Action," of October 1953 supplements several chapters in this number. The June 1956 REVIEW, "Twenty-Five Years of Educational Research," contains an excellent chapter, "Methods of Research," with a splendid book bibliography. The February 1957 issue of the REVIEW summarizes educational research in countries outside the United States and notes advances in research methodology where they exist in that literature. Finally, our parallel issue in the previous cycle, "Statistical Methodology in Educational Research," published in December 1954, is an excellent intensive review of one facet of the research methodologies field, and is far from outdated.

We hope that readers will find our coverage of familiar topics more than adequate and that the review of additional areas will contribute significantly to the improvement of educational research.

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CHAPTER I

The Philosophy of Science and Educational Research

MAY BRODBECK*

SCIENTISTS do science. They formulate concepts with which to describe the facts they find. They look for laws connecting some of these facts with others. They try to formulate theories in order to explain known facts and laws and to help find new ones.

Philosophers of science talk about science. They try to clarify the nature of scientific concepts, laws, and theories. How do concepts acquire meaning? What is the logical form of a law and of a theory? What is the meaning of explanation in science? Of causality? What is a model? Is there any connection between scientific description and the values scientists hold? These are some of the more general questions philosophers of science try to answer. They may also analyze the concepts, laws, and theories of specific sciences like physics, psychology, or biology. Thus, the logical structure of relativity theory in physics or the difference between vitalism and mechanism in biology might be such special issues engaging the attention of philosophers of science. The enterprise is clarificatory. It seeks neither new factual knowledge nor new technics but, by logical analysis, clarification of the knowledge and methods we have.

Occasionally, this analysis implies a criticism of some of the things scientists say about what they do. Sometimes this criticism is helpful to the working scientist and contributes to greater achievement. In any case, clarity and understanding never do any harm.

The less well-developed a science is, the more germane will be the general analyses of philosophy of science. Physicists may perhaps be spared a lesson in how to formulate precise, meaningful concepts. Social scientists can still profit from such lessons. Educational research is part of the less-developed social and psychological sciences. I shall, therefore, concentrate on recent analyses by philosophers of science of some more general problems of meaning and explanation in science.

Operationism

What is the *principle* of proper concept formation in science? This question is fundamental, for the kind of answer given determines also the answers to many other questions such as those about the nature of causality and of induction. In common sense, the meaning of a term referring to a physical object, like a dog or a chair, is given by listing the observable attributes of these objects, like barking and shape. But science is not concerned with the meaning of physical-object terms. It

* This paper was written during the tenure of a Faculty Research Fellowship from the Social Science Research Council. The author is indebted to the Council for the time thus granted.

takes these for granted as given by common sense. Its concern is with terms referring to rather more *abstract* properties of physical objects. The characteristic abstractness of scientific concepts, like mass or IQ, lies in the fact that these terms cannot be defined by simply listing a cluster of directly observable attributes. Merely by looking at a surface we can tell whether it is red or by looking at an object whether it is a dog. We cannot so simply tell what the mass of an object or the IQ of a child is. Yet, we must know what to look for in order to tell whether statements that an object has a given mass or a child a certain IQ are true or false. How then are these scientific words to be defined?

Operationism answered this question by making explicit what had been customary practice within physical science for generations. It pointed out that scientific terms must be defined, not in isolation as in a dictionary, but by stating the observable conditions under which a sentence containing the term is true or false. More specifically, an operational definition has the form of a conditional or *if-then* sentence. The antecedent or *if* clause of this sentence states the test or stimulus conditions or what must be done in order to make certain observations. The consequent clause states the truth or response conditions or what must be observed after the test conditions have been imposed. In the case of quantitative concepts, these test conditions consist of certain measuring procedures or *operations* such as weighing on a balance or giving an examination. The truth conditions state what must be observed after these operations or manipulations have been carried out. Terms referring to personality traits, attitudes, and abilities must clearly also be defined in terms of behavior that is exhibited under certain conditions. All terms, whether quantitative or not, requiring the if-then form of definition may be called *dispositional* concepts (3). An operational definition, like any other definition, is a statement about the use of words, stating how one term may be eliminated by means of others. Like any other definition, it is thus purely verbal or tautological.

Criticisms

Once a heroic rallying cry in the behavior sciences, operationism is now everyday good scientific practice there as elsewhere. It is not a special philosophical or methodological position. It merely clarifies the form definitions of scientific concepts must take in order to determine when statements containing these terms are true or false. As the study of man came of age scientifically, it inevitably adopted the practice of defining its terms. In fact, the practice is a necessary condition for such coming of age. Yet this modest proposal for forming empirically meaningful concepts, dignified by the title, *operationism*, has had its critics.

There has always been criticism from the right, that is, from sources essentially hostile to a science of behavior. Life and space are too short for more than one brief comment on these last-ditch defenders of lost causes. Their essentially antiscientific plaint is to the effect that definition, opera-

tional or otherwise, deprives science of the rich halo of meanings surrounding terms in ordinary use. So it does, and so it should. The scientist may draw upon this halo for hunches about laws, but if he wants objective knowledge of behavior, he cannot carry over the vagueness of ordinary usage into his technical vocabulary. Or so it would seem. Yet rather early in the game, operational definitions were attacked as unduly restrictive by sources undeniably favorable to a science of behavior (11, 14, 15). Since operational definitions are, after all, just definitions, it is not surprising that, in fact, the strictures were generalized to embrace the whole principle of empiricism that concepts to be meaningful must be defined in terms of observable properties of things. Instead of the narrow criterion of definability, it was urged that the meaning of a term must be left *open*. This openness is gradually filled in, tho never closed, as we increase our knowledge. Or, as it is also put, the meaning of a term is given not explicitly by definition, but implicitly by the set of laws or theory in which the term occurs.

Explicit Definition and the Nomological Network

What are the arguments for and the merits of the view that meaning cannot be given by explicit definition?

There are several different ways of measuring the length of an object, hunger in a laboratory animal, or the IQ of a child. If the meaning of a term is given explicitly by its definition, all these different antecedent measuring conditions result in different definitions for length, hunger, or IQ. Yet we may have good reason for believing, or even just *feel*, that these all measure the same thing. Moreover, we may continue to devise new test conditions for the presence or absence of the property in question. Any definition by means of a single test condition for the presence and absence of an attribute, therefore, fails to capture the full meaning of the term. And since we are always adding to the list, the meaning of a concept is never more than partially determined. The group of alternative criteria for the application of the term does not, therefore, literally define it. The unending list of test and truth conditions does not permit the elimination of the term. Accordingly, it is said to be *reduced* to, rather than defined by, the set of if-then sentences about the conditions when the term is applicable. The latter, in turn, are called *reduction sentences* and only partially specify the meaning of the term (11, 14).

Definability was thus liberalized into *reducibility*. The latter countenanced, if it did not advocate, terminological indefiniteness. It is perhaps not surprising that empirical reference was soon to be drowned in a sea of context. Consider certain clinical and social concepts, like anxiety or group morale. What kinds of observable behavior shall constitute the definition of such terms? Answers to a test may give a high anxiety score. But people with identical scores also exhibit many different varieties of behavior. To really know what *anxiety* means, it seems reasonable to say

that one must know not merely the test scores but all the characteristics predictable on the basis of them. Such concepts, according to the most lucid exposition of this view (12), are postulated attributes or *constructs* which are *reflected*, but not defined, by the test performance and all other behaviors positively correlated with it. Their full meaning is given by the set of laws in which they occur.

This set of laws is called a *nomological network*. The more incomplete the network, that is, the less we know, the vaguer are our concepts. Until we know all the laws in which a term occurs, we do not know precisely what it means. It is only partially defined. After we have all the laws, the term is implicitly defined by this network. Reverberating in this doctrine is an echo of the arch-rationalist Hegel. A denial that terms have referential, noncontextual meaning is the essence of Hegel's coherence theory of knowledge. Does the nomological network disguise in empiricist trappings a resurgence of the old idealistic metaphysics? In any case, the extreme view (18) that *no* term has meaning apart from a system of laws is not generally embraced by the critics of definition. The anti-empiricist implications of a view which bases meaning wholly on context at the expense of extrasystematic descriptive reference are too potent. The view is unpalatable both to the scientist who tries to describe the world and to the philosopher who tries to show how the scientist accomplishes this. Its more moderate advocates grant that some of the terms of the network are definable in terms of observable events (10, 12). Those which allegedly are not thus definable are held to be connected to these by chains of laws. They are thus *partially coordinated* to the realm of experience.

Logical and philosophical technicalities apart, this is the gist of the arguments against explicit definitions. To some, they are convincing. Others find them irremediably confused (3, 4). In particular, the formula that "the meaning of a concept is the set of laws in which it occurs" is charged with confusing issues of fact with those of meaning. This confusion is abetted by an ambiguity in the term *meaning*.

Meaning and Significance

In one sense of that word, *meaning* is something we give to a concept when introducing it for the first time. It is what we agree to call a certain attribute or cluster of attributes. Thus it is purely verbal or, as one says, a matter of convention that *dog* refers to a barking rather than to a meowing creature. On the other hand, the generalization or law that "dogs are carnivorous" is a matter of fact and not a matter of the way we use words. Yet, on the nomological-network view, meat eating is part of the meaning of the concept dog. As we know more about dogs, we expand the meaning of the concept.

To be sure, in one sense of meaning, a concept is more meaningful to us the more we know about it. But what is the *it* about which we know more and more? This can be answered only by distinguishing between two different meanings of *meaning* (2, 3). One of these is the empirical referent

for which the term is introduced. This is purely verbal. The other refers to the set of laws in which a term occurs. This is an empirical matter. The word *significance* has been suggested for the latter. A term may have meaning without significance, but not conversely. Can we not add the new things we find out about something to its definition, thus giving more meaning to the term? Up to a point, we can. Suppose that the results of three different tests for intelligence are found to be concomitantly connected. *Intelligence* is now redefined, so that it means not any one of these test results, but all three together. The term has been redefined to keep up with our knowledge, but what has been gained by doing so?

Suppose *intelligence* now means a certain level of vocabulary, arithmetic ability, and general information, or, perhaps, something presumed common to all three, like linguistic ability. Yet, in order to be able to predict that an individual with high vocabulary is also good at arithmetic, we would still have to state separately the empirical law connecting these attributes. So nothing has been gained by packing everything we know into the meaning of the term. Suppose it happens that *status* as initially defined in one way and *intelligence* as defined in another are found to be connected. Do we want to coin a new word whose meaning will include both the referent of status and that of intelligence?

This is the logical conclusion to which the notion of partial definition carries us. Yet it leads nowhere. Without independently asserting the empirical law connecting status with intelligence, the new concept is just a word for a cluster of characteristics. Without the statement of the law, we have no justification for stating that members of this cluster uniformly occur together. Nothing follows from definition or meaning alone. Only from laws can we make predictions.

Furthermore, how could we have discovered that intelligence is connected with status unless these terms have independent meaning? Again, *what* is it that we have knowledge *about?* In other words, in order to discover the significance of a term, we must first know its meaning. Significance alone will never give us empirical science. If the system of laws is to be about the word—if it is to be a factual, descriptive system—its concepts must also have meaning in the sense of designation or reference.

Nor can laws intelligibly be said to implicitly define their terms. The term *implicit definition* is a most misleading manner of speaking (8). If it makes sense at all, it makes sense only when one is speaking of the axioms of a formal system, that is, a system of marks on paper containing expressions like "X and Y," to whose letter variables no meanings have been attached. For such a system, the axioms implicitly define its terms only in the sense that, showing structure tho no content, they delimit the range of possible meanings or the interpretations that can be given to the symbols of the system if true statements are to result. Replacing the letter variables by some empirical concepts gives true sentences. When replaced by others, the resulting sentences will be false. But there may be many alternative sets of empirical terms which will give either result.

The term makes no sense when, as in empirical science, our system contains not uninterpreted letter variables but empirical concepts, theoretical or otherwise.

What of the technic of partial interpretation, or of partially coordinating so-called theoretical constructs to terms that do name observables? This procedure was suggested by the example of atomic and subatomic physics, the only place in physical science where terms are introduced in this way. But it is the axiomatic system in which these atomic notions are embedded which is partially coordinated, not the atomic concepts themselves. Some of the terms of this axiomatic system are coordinated to those of another system, like thermodynamics, all of whose terms are empirically defined. On the other hand, some of the terms, like *electron* and *mass of electron*, are not coordinated at all. They do not have partial empirical meaning. In this very special sense, they have no empirical meaning at all. The situation is a very special one indeed (3).

The uncompleted network of behavioral laws in which the theoretical terms occur will presumably come to include terms referring either to further behaviors, or to neurophysiological, chemical, or other such events. In any case, these other terms would, if they were not leaning on each other in the curious circular manner of the nomological network, refer to observable attributes. No matter which way you look at it, the behavioral scientist's candidates for theoretical constructs, like *hostility, aggression, morale,* and the like, must have *some* referential meaning. If, on the one hand, meaning is confounded with significance and the meanings of these terms are given by the laws in which they occur, then for there to *be* any laws, these terms must have independent meaning. On the other hand, if these terms have no independent meaning, their connection to observables can be only verbal or definitional. Their empirical meaning is then given by this definition, which may, of course, be changed.

Definitions, being merely verbal, are tautologies. Laws are empirical statements. If the latter are absorbed into the former, if everything is made a matter of meaning, we have not an empirical science but a structure of tautologies. This would make nonsense of the whole enterprise. How then does one come to hold this view? Practically speaking, behavioral scientists often cannot define their terms precisely. The precise definition of social attitudes or clinical states requires one to choose from an almost infinite variety of *symptoms* those which can be used reliably to define the term in question (8, 9). The list of behaviors which together enable prediction to other behaviors cannot yet be sharply terminated. Such terms thus have a fringe of vagueness or *openness.* To achieve greater reliability and significance, such terms are frequently redefined to include new factors or, for that matter, to drop out old ones. Loosely speaking, we say the original definition was only partial. Accurately speaking, we frequently abandon our definitions and propose new ones. The initial vagueness and consequent frequent redefinition is part of the hit-and-miss way a science progresses. But the logic of science is concerned with the *principle* for

good concept formation. The psychology of discovery is something else again. The purposes of neither science nor logical analysis are served by exalting the exigencies of research in a difficult area into a methodological principle.

Causation

What is the difference between a *true* and a *spurious* correlation? What is the difference between a causal connection and a merely *accidental* conjunction of events? Despite immediate appearance to the contrary, these are not quite the same questions. Researchers accustomed to working with statistical correlations have developed technics for distinguishing the true from the spurious correlation. A high correlation, for instance, between female marital status and job absenteeism is said to be true, while that between marital status and candy consumption is called spurious. In the latter case, the introduction of an additional factor, age, leads us to abandon the original correlation. In the true case, on the other hand, the introduction of an additional factor, increased housework, is said to confirm the correlation. Why, in each case, do we treat the original correlations differently after introducing the additional factor?

After all, marriage is statistically correlated with age, thus also with candy eating. Statistically, therefore, in both cases there actually is a correlation and both are *explained* by the third factor. Married people eat less candy because they are older; married women are absent more from jobs because they have more housework. We justify saying that nevertheless the former correlation is spurious and the latter is true, because we analyze the notion of a true correlation in terms of a presumed causal connection. Getting married causes more housework, which in turn causes increased absenteeism, so getting married is truly correlated with absenteeism. Getting older, on the other hand, is a common cause both of marriage and eating less candy. All concomitants of age, like grey hair and paunchiness, would give a high correlation with eating less candy, if age does. They have a common cause, but are not causes of each other. Thus, the difference between true and spurious correlations resolves into a difference between causal and noncausal connections.

Nor does the difference between causal and noncausal conjunctions arise only for statistical correlations. Nonstatistical generalizations, asserting for all things of a certain kind that they are uniformly connected with something else, also raise the same problem. "All gases expand when heated" states a true causal connection, while "All the books on my desk are blue" does not. Philosophers have puzzled about how to distinguish the truly causal connections from those which are merely accidental. In particular, it has been pointed out that the usual formulation of an empirical law as an if-then statement does not reveal this distinction. Both the real law and the accidental conjunction would each be expressed as conditional statements. If anything is a book on my desk, it is blue; if anything is a heated gas, it expands. The conditional states the observed constant conjunction to these characteristics.

The analysis of statements like "A causes B" into statements about a uniform conjunction of events, without using the term *cause,* has been, ever since David Hume, basic empiricist doctrine. This analysis follows from the empiricist criterion of meaning, of which operationism is merely an application. All that we observe is the constant conjunction of the events "A" and "B" and not a third thing called a cause.

Idiomatically, we may distinguish between accidental and causal connections by using the subjunctive mood. If this gas were heated, it would expand. On the other hand, if a book in the bookcase were on my desk, it need not be blue. It has, therefore, been suggested that only by means of the subjunctive can we distinguish lawful connections from mere generalizations (13, 17). "A causes B" or "If A then B" is an empirical law only if we can truly assert the corresponding subjunctive, "If anything *were* A, then it *would be* B." To put it differently, if the corresponding subjunctive is true, then we have a real connection, otherwise only an accidental generalization. This seems a rather neat solution. Unfortunately, it has some obvious difficulties.

How are we to *know* the truth or falsity of the corresponding subjunctive? Fundamentally, there are only two alternatives. One is that we know it by inductive generalization from observation. But we observe only that whenever we have A, we also have B. The subjunctive, therefore, can be asserted only on the basis of prior knowledge of the indicative conditional. But then the subjunctive is superfluous since the evidence for it is no different from the evidence for the corresponding indicative conditional. The alternative is that we know the truth of the subjunctive in some special way. The empirical evidence for both the causal connection or true empirical law and the accidental conjunction is never more than a finite number of instances. There are thus no observations distinguishing the truth of the subjunctive from that of the indicative form. If, therefore, the subjunctive says more than the corresponding indicative and if this excess meaning is not further analyzable, we must know it in some special way.

We must somehow grasp or see that one subjunctive is true while another is false. On the empiricist analysis, a law of nature is expressed by the indicative if-then form. Rejection of this analysis leads us down the path of rationalistic intuition or reason. I mentioned before that one's principle of proper concept formation or criterion of meaning was fundamental. We see now why this is so. On the unanalyzable subjunctive view of empirical laws, we are, in effect, back to an unanalyzed notion of cause and to rejection of the empiricist criterion of referential meaning. Inductive generalization gives way to intuitive grasp of real connections. Is this really the price we must pay for the ability to distinguish between lawful and accidental uniformities? Clearly, this is a distinction we should like to be able to make. Fortunately, this can be done without sacrificing empiricist views on meaning and knowledge. But the distinction cannot be made simply by considering generalizations by themselves, in isolation (3, 6, 7, 20).

The difference between a law and an accidental conjunction of events is a matter of fact and not of meaning. For matters of fact, it is reasonable to point out that we must look at the context, that is, at the rest of what we know. Let us reanalyze the difference between the spurious correlation between female marital status and candy consumption and the true correlation between female marital status and absenteeism. Why in this latter case does an additional factor, increased housework, confirm the correlation? The answer can be given without the use of cause. Introducing the third factor, more housework, leads to two new generalizations: When a woman marries, she has more housework, and if housework increases, so does absenteeism. From these two generalizations, the correlation in question between marriage and absenteeism follows as a deductive consequence. It is thus explained by them, in the only precise meaning *explanation* has in science. Because we can explain the correlation by deducing it from other generalizations, we consider it to be a true one. On the other hand, in the spurious case, the additional factor, age, does not permit such deduction or explanation. Again we have two new generalizations, namely, age correlates with marriage and age correlates with candy consumption. But from *these* two generalizations, all that logically follows is that age is correlated both with marriage and with candy consumption. We cannot derive the correlation between marriage and candy consumption. When we define *explain* precisely, we see that the new factor, age, does not explain the correlation. That is why it is abandoned as spurious.

Theories

A theory is a deductively connected set of generalizations. A generalization is a law if it is part of a theoretical structure. The generalizations serving as premises are laws because they permit the derivation, hence the prediction and explanation of other laws. If a generalization either predicts or is predicted by other laws, the evidence for it is more than the mere conjunction of its observed instances. It is for this reason that we state firmly that if a gas were heated, it would expand. We assert the subjunctive because the law about the expansion of gases is not due to mere enumeration of instances. On the other hand, neither is it due to any unanalyzable connection between temperature and expansion. Rather, we believe this to be more than a mere conjunction because it is part of the theory of thermodynamics. It both implies and is implied by many other highly confirmed statements. Until we know more about how an isolated correlation is connected with other facts and generalizations, we cannot tell whether it is true or spurious, to use the statistical jargon. The decision about whether we have a law or an accidental conjunction thus depends upon further empirical knowledge and is not a matter of intuitive insight of grasp of real connections among things.

Models

The recent literature of behavioral research is replete with *models*. The time is clearly more than ripe for a thoro logical analysis of their nature and function. Yet, such attention as philosophers of science have recently given to models is disappointing. Except for one highly technical treatment (6), philosophical discussions, like those of scientists themselves, are rather more hortatory than clarificatory. Models, we are assured, are the "core of discoveries" (19). How, and in what sense, are they the core? Beyond reiterating that they provide a way of conceiving or thinking of phenomena, a way of speaking (16, 19), no real clues are offered to the logical connections between the model and the theory for which it is a model. Nor are we told precisely how it is that models help us explain, beyond apparently providing a feeling of familiarity. Optics, we are reminded, uses a geometrical model. It deals with optical phenomena by the use of geometrical pictures. But what exactly is the connection between geometry and the physics of light rays? Are the pictures and diagrams really an essential part of the model? How, *in general*, can the laws of one area, like geometry or physiology, be a model for the laws of another area, like optics or psychology respectively? Is the term *model* always used in the same way? How do models differ from theories? Questions like these and many more must be answered, if we are to understand the nature and function of models.

The fact is that the term *model* is used most ambiguously. Nor is *mathematical model* any more precise since this term, too, covers different things. Broadly speaking, there are two major uses of *model*. The most general use is as a synonym for theory. A scientific theory is a deductively connected set of laws or generalizations, some of which, the axioms, logically imply others, the theorems. A theory may be well or ill confirmed, narrow or broad in scope, quantified or nonquantified. *Model* is now frequently used for those theories which are either highly speculative or quantified, or, most likely, both. Thus, a guess about the connections between quantified variables of an area, like psychology or economics, will frequently be called a mathematical model. Such hypotheses are mathematical only in the sense in which physics is mathematical. That is, they are empirical generalizations whose variables are quantified, so that we can say how much one variable changes with changes in others. They share the virtue of all quantified theories in permitting more precise deduction and prediction.

Quantification, however, is no guarantee of scope. In areas where behavior depends upon many different variables, we may indeed pay for quantification with triviality. But then, nonquantified guesses at theories, like the doctrines of psychoanalysis or speculations about the physiological concomitants of behavior (which are often broader than quantified theories but lack their precision) are also frequently called models. Such speculative theories, whether quantified or not, are after all just theories. The

term *model* serves no particular purpose beyond, perhaps, emphasizing the tentative, unconfirmed nature of the hypotheses in question. This usage would be harmless enough if it were not the case, as it unfortunately is, that there is another, quite different prevalent use of the term.

Isomorphism

Strictly speaking, I should have said two further uses (9). For in this second meaning of the term two different things are really involved tho they have a common feature. This feature I shall now explain. A miniature train is a model of a real train if it is isomorphic with it. Isomorphism requires two conditions. First, there must be a one-to-one correspondence between the elements of the model and the elements of the thing of which it is the model. For every chimney stack, there is a miniature chimney stack; every window has its replica, and conversely. Second, certain relations are preserved. For instance, if a door is to the left of a window in the original, their replicas are similarly situated; also, the model is constructed to scale. If the model works on the same principle as the original—if, for instance, a model steam engine is also steam propelled—the isomorphism is complete. Extending this notion to theories, a precise meaning of *model* may be formulated. The form of a law is given either by the verbal if-then formulation or by an equation. If, for example, weight is a linear function of height and if supply is a linear function of demand, then these laws have the same form tho different content. The content is given by the empirical terms. Two theories whose laws have the same form are isomorphic or structurally similar to each other.

If the laws of one theory have the same form as the laws of another theory, one may be said to be a model for the other. This is the second most general meaning of the term. The laws of one area may suggest hypotheses about the form of laws in another area. The notion of *model* as isomorphism of laws is obviously symmetrical. However, when an area about which we already know a good deal is used to suggest laws for an area about which little is known, the familiar area providing the form of the laws may be called a model for the new area. Thus, the biological theory of evolution may be used as a model for social theory. Servomechanisms, like the automatic pilot or thermostat, are now frequently evoked models for learning and purposive behavior.

Testing Models

How does one test these suggested models? First, it must be possible to state clearly what is in one-to-one correspondence with what. Organisms grow; that is, they increase in size and weight. What is *social* growth? Relatively precise meaning can be given to adaptive and nonadaptive characteristics of organisms within evolutionary theory. Can we give correspondingly precise meanings to these notions for human institutions? Once clearly defined empirical concepts are made to correspond to the terms of the model, structural similarities, if any, are sought. Nutrition

is connected with growth in biology. Are the social concepts corresponding to nutrition and to growth similarly connected? In other words, not only must the terms of the two areas correspond, but the connections between them must also be preserved if the model is to be of any use. An area, either part or all of it, can be a fruitful model for another only if corresponding concepts can be found and if at least some of the laws connecting the concepts of the model also can be shown to connect their corresponding concepts.

Arithmetical Models

Replacing all the empirical, descriptive concepts in the theory of one area by those of a different area results in another theory with the same form but content different from the original. The isomorphic sets of laws, those of the model and of its *translation*, are both empirical theories whose truth or falsity depends upon the facts. It is possible, and often highly desirable, to establish another kind of isomorphism, in which the result is not two empirical theories sharing a common structure. Instead, the laws, or some of them, of an empirical theory may have the same form as a set of purely arithmetical truths. If this is the case, the latter is called an *arithmetical representation* of the empirical theory. *Mathematical model* sometimes means just this sort of arithmetical representation of an empirical theory. The laws of arithmetic, rather than those of another empirical theory, may be used as a model when, for instance, it is desired to rank or measure the variables of an area. If, like the integers, the empirical terms of an area obey the axioms of order, they may be ranked. For instance, one of these axioms states the transitivity of the arithmetical relation, *greater than*, among integers. This means that if one number is greater than a second and the second is greater than a third, the first number is also greater than the third. Replace the integers by names of individual people; replace the arithmetical relation, *greater than*, by the empirical relation, *smarter than*. If the statement resulting from this translation is true, the empirical relation of one person being smarter than another is transitive. If, in addition to this axiom, *smarter than* also satisfies the other axioms of order, individuals may be ranked by this relation.

If the variables of an area are quantified and obey not only the axioms of order but also further axioms for the addition of integers, measurement is also possible (1, 5). Measurable descriptive properties are those having the same structure as the addition of numbers. The measurability of descriptive properties is expressed by a set of empirical laws which are isomorphic to the laws of arithmetic. By virtue of this isomorphism, numbers may be assigned to the properties of things, resulting in quantified empirical laws. Other parts of arithmetic serving as models for empirical properties are, for example, probability theory and the theory of games (9). A correspondence is established between the empirical concepts and those of the arithmetical theory.

Whether or not true empirical laws result from this correspondence depends upon the facts. Many properties are not transitive and thus cannot be ranked. Many properties are nonadditive and thus cannot be measured. If, however, the empirical variables do share the same structure as the laws of arithmetic, all the arithmetic theorems can be used to make deductions from these quantified laws to other laws and facts. When a model, either empirical or arithmetical, is used as a source of hypotheses about the connections among the variables of another area, it does not explain these hypotheses. It merely suggests their form. If, however, these new hypotheses are confirmed, they may be used to explain and predict new knowledge.

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CHAPTER II

Research Methods: The Cross-Cultural Method

GEORGE W. GOETHALS and JOHN W. M. WHITING

A GOOD method, to paraphrase Guthrie, is at best a tool by which explanation is furthered. In the final analysis a theory or method stands or falls upon its utility. The cross-cultural method presents a number of paradoxes which the reader must keep in mind as he considers it as a possible tool for research. First, altho the method has been in existence for some 70 years, not until recently has either its scope or its usefulness been recognized. Second, unlike many areas of research, there are relatively few studies which have been published utilizing this particular method. An evaluation of a methodology usually is based upon a large amount of published research which the reviewer can refer to and evaluate. Such is not the case with this particular method. However, despite the paucity of published research this particular method has had a great effect upon behavioral science, and in relation to recent work may have shown itself to be the most sensitive methodology available for those who wish to accomplish interdisciplinary research in the behavioral sciences. Since this is the first article dealing with the method in the REVIEW, some recapitulation of its history is appropriate. This is particularly true since the cross-cultural method is often confused with another valuable approach to the study of behavior, namely, the comparative or culture-personality study.

Historical Background and General Discussion

To gain perspective, it is interesting from a historical point of view to note that this method antedates the birth in 1912 of modern psychology as delimited by Woodworth (44) and Boring (7) and the advent of modern clinical psychology usually identified as beginning with the work of Binet in 1895 (33). The cross-cultural method can best be understood if two facts are kept in mind. First, since the first study by Tylor (37) in 1889, this has been a method employing statistical technics *to test theory*. Second, while the cross-cultural method in recent times has been concerned with matters of personality development in different cultures, it differs markedly from the comparative study because of its insistence upon testing theoretical positions.

The work of Honigmann (18), Kardiner (20), Kardiner and others (21), Mead (27), Mead and Metraux (28), and Hallowell (14), while generally highly sophisticated in its approach to the investigation of the theoretical problems, has in common one dimension which clearly separates it from cross-cultural research. This is that these comparative studies consistently concern themselves with determining whether a particular hypoth-

esis, derived from some segment of theoretical insight, can be fitted into some more general cultural framework. The implication of such a technic is that the investigator accepts without question the particular theory under discussion and is exemplifying it upon a broader canvas. The few quantitative facets of such studies which do exist have to do almost entirely with determinations of normative or modal behavior.

The cross-cultural method, on the other hand, is always concerned with the test of some theory. Generally it uses quantitative technics described by Stephenson (36) as "R methodology," and further, these are in keeping with designs derived from the thinking of Fisher (10). Very simply, these technics are concerned with testing the significance of some form of correlation. If this difference is kept in mind at the start, much of the confusion between the two technics will be avoided. Those who are interested in detailed analyses of the difference between these two useful approaches are referred to work by Allinsmith and Goethals (1), Lewis (24), McClelland (25), Murdock (30), Whiting (39), and Whiting and Child (40).

Cross-Cultural Studies Testing Aspects of Evolutionary Theory

The first cross-cultural study was read at a meeting of the Anthropological Institute of Great Britain, presided over by Sir Francis Galton. This was a research by Tylor (37) investigating the development of laws of marriage and descent. The study was concerned with testing constructs derived from the theory of evolution. In 1915 Hobhouse, Wheeler, and Ginsberg (16) completed a study testing hypotheses which predicted relationships between certain social institutions and stages of economic development. After another lapse of 25 years Simmons (34) and Murdock (29) investigated other constructs relating to kinship derived from evolutionary theory. These studies are important in that their sophistication and use of newer statistical technics foreshadowed later studies.

Cross-Cultural Studies Testing Behavioral Theory

During the past 20 years there has been growing interest in the interdisciplinary responsibility for developing a general science of behavior, as evidenced by attempts to bring together the most profitable and rigorous ideas from the fields of anthropology, psychoanalysis, and experimental psychology. One of the best reviews of such efforts was presented by Allport (2). One of the first to see the possibilities of this rich body of theory was Ford (11, 12). His study of human reproduction, because of his normative treatment, is not in the strictest sense of the word a cross-cultural research; however, his methods of arriving at ratings of various

behaviors in different cultures have been used in one way or another by all succeeding cross-cultural researchers (13).

Within the limits of our definition, that is that a cross-cultural study must test some hypothesis, the first study testing behavioral theory was that of Horton (19). He employed the cross-cultural method to investigate the relationship between the drinking of alcoholic beverages and anxiety. His findings revealed that alcohol reduced inhibition in all societies unless specific measures were taken to prevent it. Other researches followed; the most complex was Murdock's fascinating work (30), published in 1949, which tested many hypotheses concerning kinship terminology and its relation to such phenomena as forms of marriage, descent, and social structure generally.

Whiting's early study (38) reporting the relationship between sorcery and social control is of great historical importance. The steps that Whiting took in constructing her research foreshadowed the more sophisticated methodology which was to become part of later studies, and at the same time attempted to answer some of the original criticisms of the method. First Whiting, on the basis of her field work with the Paiute Indians and from behavioral theory in general, derived a number of hypotheses having to do with the function of sorcery as a means of social control. Once these hypotheses were stated, the next step was to examine the same variables in a number of other cultures and to test to see if they had the same relationship they had had in the original single culture. One of the important aspects of the study was that she tried to rate independently the variables under consideration and thus to make sure that any relationship between them was a result of a theoretical interaction rather than rater bias (42). She also undertook to control the geographical distribution of her sample of culture so that her findings could not be attributed to cultural diffusion. This progression from the single case to testing hypotheses derived therefrom, with reference to a sample of cultures, and then controlling for the contamination of the findings as a result of geographical concentration reveals the eventual design of later cross-cultural studies.

Behavioral theory has given rise to a number of studies all marked by their richness and rigor. Barry (5) tested the relationship between the education of the child and art forms. Wright (45) demonstrated the relationship between the content of myths and education as these both relate to aggression. McClelland and Friedman (26) demonstrated the relationship between certain child-training variables and need achievement. Finally, in 1953 Whiting and Child (40) showed the relationship between various technics of education and the development of super-ego and other manifestations of personality.

As provocative as these early studies have been, they only hint at the limitless possibilities of the cross-cultural method. The methodology did not come of age until recently; this was not possible until certain flaws in the technics were rectified.

The Problem of Data

Whiting and Child (40) in their elaborate study relating socialization pressure in different societies to facets of adult personality, described precisely how data are collected and utilized in any cross-cultural study. Despite this lucid description it is interesting that most of the criticisms of the cross-cultural method have been focused not so much upon the method itself, as upon the data various investigators have used. This criticism evolves from two preoccupations, one within and one outside the discipline of anthropology. Both of these dimensions are of concern and demand attention. Within anthropology, Spiro (35) and Kluckhohn (22) pointed out that the report of the field worker upon any given culture is no little affected by his conceptions of what anthropology as a science includes. Any given individual thus views a culture in terms of his background and training, and unless these are constant, any report of what is seen is open to the widest amount of variation. Criticisms by Lewis (24) and by Henry (15) concern a matter general to any investigation of behavior and relate to the control that can be introduced into any field investigation.

There is no doubt that these observations have some validity; criticism of the use of ethnographic sources written at different times by people with a variety of backgrounds and personal predilections is a telling one. As a criticism, however, it would be far more serious if the practitioners of the cross-cultural method had shown themselves to be unaware of this difficulty and even more important had done nothing to remedy it. Many of the reservations concerning the cross-cultural method can be put into proper perspective when it is realized that no group has been more aware of the limitations of their data than those who have employed the method.

Obviously, ethnographies already in existence cannot be completely rewritten. However, such materials can be brought up to date in relation to a strict set of criteria. The publication in 1950 of the *Outline of Cultural Materials* (31) by the Human Relations Area Files provided a method of organizing the materials and for correcting some of the deficiencies which exist. The introduction to the *Outline* provides the person working in an applied field, such as education, an excellent overview not only of the processing of data but also of its application to everyday problems.

However, the best possible way to answer the criticisms brought forward both by anthropologists and behavioral scientists in general was to undertake a program which answered both kinds of criticisms, that is, provide training so that a group of anthropologists could collect field data in the same way and provide training in the use of the method *before* going into the field. Investigators for three universities, Cornell, Harvard, and Yale, financed by grants from the Ford Foundation and the Social Science Research Council, have jointly since 1952 undertaken this dual task. From 1952 until 1954, teams were trained before going into the field to undertake an investigation of socialization in five different cultures: in New England, Okinawa, Mexico, the Philippines, and in a Hindu culture. These field

teams returned in 1955 and since that time have been analyzing the data collected under these carefully controlled and comparable conditions. The critics of the cross-cultural method will find most of their objections answered by a perusal of the field manual which specifies in detail the variables of the research and their method of measurement (43).

The universality of the method is naturally its recommendation, and the kinds of observations made are consistent with one of the most intensive studies ever done on socialization in American culture (32). These studies of the five different cultures will be published within the next few years and will exemplify the cross-cultural method at its newer level of sophistication.

Recent Changes in Scope and Method

As these new safeguards relating to existent data and the collection of new information were being undertaken, cross-cultural researches began to go forward into new areas. Hollenberg (17) and Faigin (9) tested a number of hypotheses relating to technics of education and the development of the super-ego from a sample of three cultures in the Southwest. In this study, as in the comparative study of values in five cultures directed by Florence Kluckhohn, John Roberts, and Evon Vogt of Harvard University, the methodology is modeled on that used in conjunction with ethnographic literature but has been broadened to include field research. The approach is not the same as the elaborate collaboration previously described, but it, too, has the advantage of increasing the comparability of data and at the same time testing and retesting hypotheses based upon individual differences in dissimilar cultural settings.

Another advance of the cross-cultural method within the scope of ethnographic materials is exemplified by the work of Ayres (4), Anthony (3), and Whiting, Kluckhohn, and Anthony (41). Essentially these studies are concerned with the interaction of various sets of variables rather than with a strict antecedent-consequent relationship between two variables. Ayres, for example, was concerned with the relationship between pregnancy taboos, family structure, and dietary regulations. Anthony was concerned with showing how initiation ceremonies were related both to child training practices and to aspects of the kinship organization of various cultures.

The way Anthony went about his study is as important for this new kind of research as the steps Beatrice Whiting took in her study of sorcery. Anthony began with a construct from psychoanalytic theory discussed at some length by Bettelheim (6), who observed that certain cultures put young males thru extremely painful initiation rites before permitting them the status of men. Usually these are related to some painful operation involving the genitals, and typically the ordeal involves public circumcision without anesthesia. The behavioral scientist refuses to admit that such a traumatic event exists for capricious reasons. Bettelheim had seen this "symbolic wound" as being a way both to introduce the young male to

adulthood and to control his access to women before maturity. Anthony proceeded to test this hypothesis and found that there was indeed a striking relationship between the *rite de passage* and the social organization of the group in which it took place. Unexpectedly, however, he found some important relationships between kinship, education, and the forms of marriage. The tendency of a good cross-cultural research to generate new hypotheses is thus exemplified in Anthony's research.

Whiting and Kluckhohn, drawing upon the work by Anthony, showed the intricate relationship between ceremonies of initiation and many aspects of the social milieu which exists around them. Most important to the field of education is the light shed upon the variations in the intensity of the adolescent revolt, a phenomenon that is shown to be widespread. The ways in which other societies cope with this dilemma can help us gain new perspectives on the problems of juvenile delinquency and the modes of transition from childhood to adulthood.

Implications of the Cross-Cultural Method for Educational Research

The advantages of the cross-cultural method to the field of education are at least two: First and foremost it forces upon education the realization that there is a broad range of methods by which a child can be brought up. At the same time it shows that these different methods are neither chaotic nor "primitive" as some of the earlier reports of "strange happenings in the South Seas" would have indicated. Instead, child-rearing methods are functionally bound in a meaningful way to other parts of the life plan of the society. Polygamous societies, for example, and monogamous societies such as our own, have been shown to differ systematically in their methods of bringing up children. Further, these differences are not arbitrary or capricious but are consistent with findings derived from the current development of behavioral science which is the theoretical framework of the modern cross-cultural research. The second advantage to understanding the cross-cultural method is that above all else it causes us to be aware both of the virtues and of the limitations of the untrained observer, and much more important, it offers us ways of training people to look at the phenomena of behavior with a strategy of reason, logic, and objectivity.

Such training and such perspective are important for education now as never before. Education in this country is committed not only to training the mind but essentially to socializing the person. The school faces problems involving the emotions not only of the individual child, but also of groups of children as they come together. The field of education can no longer afford the luxury of being "culture bound." It must accept, instead, the responsibility of knowing the best there is in research so that the new responsibilities before it may be accepted intelligently and thoughtfully. The authors are not suggesting, nor do they mean to imply, that the cross-cultural method is an answer to every research problem in education. They

are suggesting, instead, that by seeing the evolution of this particular method and the painful steps thru which it progressed, others now concerned with constructing a methodology adequate to the complexities of education may, by a review of what has been done in this area, avoid some mistakes thru knowing of the experiences of others. Finally, by seeing this progression, the educator may come to agree with Cumming and Cumming (8) that there is nothing more practical in an applied field than the possession of good theoretical research.

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CHAPTER III

Research Methods: Experimental Design

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In his chapter three years ago, Kogan (41) illustrated a variety of experimental designs by referring to 52 studies. My initial selected bibliography contained 214 items, the seemingly most pertinent 83 of which are reported here.[1] I neither repeat any of Kogan's references nor cover material summarized by Gardner (30) and Moses (54) in their excellent chapters.

Current Trends

Modern experimental design is less than half a century old. It's grandfather was William Gossett ("Student"); its very active father, Sir Ronald Fisher; its bible, Fisher's *The Design of Experiments* (27). Recently two competitors, decision theory and information theory, have risen to challenge the supremacy of the analysis of variance and covariance. Fisher (28:69) spoke out against "the attempt to reinterpret the common tests of significance used in scientific research as though they constituted some kind of acceptance procedure and led to 'decisions' in Wald's sense. . . ." Pearson (59) and Neyman (55) replied to some of his criticisms. Cochran (15) stated that it would be unduly restrictive and harmful to view the function of statistics wholly in terms of decision making.

In a technical paper, Lindley (45) suggested that altho undoubtedly one reason for experimenting is to reach decisions, another is to gain knowledge about the state of nature in the information-theory sense of Shannon. He introduced a measure of the information that an experiment provides and formulated a rule of experimentation: Perform that experiment for which the expected gain in information is greatest and continue experimenting until a predesignated amount of information is attained.

Garner and McGill (31) compared uncertainty analysis, following Shannon, with a two-way nonorthogonal analysis of variance and concluded that while the two are similar in many respects, uncertainty analysis should be used when the criterion variable has the properties of Stevens' nominal or ordinal scale, while the analysis of variance must be employed when one desires to retain information about the metric and conditions for an interval or ratio scale are met. Frequently it may be desirable to do both analyses and compare the results.

Box (5:975) recorded his opinion that "outside the field of agriculture the sequential situation is by far the most common one." Altho the mathematical statistics of sequential experimentation have proved formidable, Grundy, Healy, and Rees (32) presented a solution for a simple

[1] A partially annotated mimeographed list of the other 131 may be obtained free from the author while his supply lasts.

version of the two-stage experiment: After the first experiment the researcher decides whether or not he needs to perform a second experiment whose extent depends upon the results of the first. Johnson (38) utilized sequential procedures to discriminate between two hypotheses about the ratio of variance components in a simple one-way classification. Ray (61) published tables for sequential tests applicable to the one-way classification and randomized blocks.

In a clear, explicit, heuristic article, Box (4) proposed a method of process improvement to be run in the normal course of production by plant personnel themselves, whereby industrial processes regularly yield information on how the product can be improved. Educators might be repaid amply for time spent pondering the applications of Box's concepts to educational "products."

Fundamental Books

Cochran and Cox (16) expanded their 1950 edition by 36 percent to cover recent developments. This expository handbook of designs is invaluable to the experimenter who already has a year or so of background in statistics. Snedecor's revision (67) should be quite helpful to educators who can translate agricultural examples into the jargon of their own areas. Davies (22) edited a large volume that, despite its title, has much relevant material; principles of experimental design are widely applicable. Likewise, the contributions of Bennett and Franklin (3), who devoted 280 pages to the analysis of variance and design of experiments, transcend the chemical industry. Perhaps Federer (24) tried to cover too much without presenting designs plans except in his examples, thereby making his treatment overly concise and difficult for most educational workers. Finney (25) wrote clearly but abbreviated excessively, providing little detailed help with computations. Ostle (57) included chapters on the analysis of variance and covariance and experimental design. A second edition of the Wishart and Sanders (82) manual appeared.

Pearson and Hartley (60) tabled percentage points of the F distribution for the .25, .10, .05, .025, .01, .005, and .001 levels of significance and for the largest variance ratio, besides providing many other useful statistics.

Applied Books

McNemar (48) expanded his coverage of the design and analysis of experiments, especially with respect to statistical models. Cornell's fresh new approach (19) merits adequate tryouts in one-year sequences; this book is a long step ahead of the usual textbook in educational statistics, but its sections on the analysis of variance demand supplementation along lines suggested by Stanley. A similar warning applies with even greater force to

Guilford's revision (33). The chapter by Edwards (23) sets forth elementary principles of experimental design.

Brunswik's "representative design" (9) bears a certain resemblance to recent work on random factors and variance components (20, 64, 81) and therefore might be pursued profitably in those terms.

Primarily Expository Articles

Baker (1) described a systematic method for arranging and analyzing the results of factorial experiments that gives the mean square relating to each degree of freedom. It is applicable either to qualitative or to quantitative factors and is especially suitable for use with a desk calculator. Stanley (70) emphasized the importance of design, stressed random assignment, and outlined the planning of two classroom experiments. Cochran (14) commented about what may well have been the largest experiment ever conducted, listing important factors in the polio field trials. Stanley (73) showed in detail how to analyze scores from counterbalanced examinations, explaining Latin and Greco-Latin square crossover designs. Campbell (11) set forth clearly and systematically considerations of group control design, with special attention to the role of pretests.

Four Basic Articles

Four long, well-written, clarifying articles of great importance to experimenters, tho probably not easy reading for most educators, deserve careful study. Scheffé (64) surveyed the current state of the theory of alternative models in the analysis of variance, showing (on page 259) expected values of mean squares for the mixed model with dependent interactions. His results agree with those of other recent investigators (20, 81) but not with certain earlier recommendations. McNemar (48:309), for instance, included in his expected mean square for the random effect in a mixed model an interaction component of variance that Scheffé omits, thereby leading the former to a more conservative test of significance for the random effect.

Cornfield and Tukey (20) dealt with average (expected) values of mean squares for several types of factorial designs, stating that they are absolutely essential to the choice of an error term—tho not sufficient, of course—and found that the customary expected mean squares (64: 259; 81: 963) resulted from their derivations under very general assumptions. Thus it may make sense for Hoyt (35) and others to talk in terms of variance components (or rather, of intraclass correlation) for dichotomously scored test items.

Wilk and Kempthorne (81) continued their contributions to the analysis of variance, defining experimental units as "those entities in an experiment

to which treatments are assigned at random" (page 951) and suggesting that the term be extended to include periods of time, states of mind, and other poorly defined complexes of conditions. They stressed the need for random sampling of "levels" of a factor when the experimenter wants to generalize to all levels, just as one samples randomly from a population of experimental units if he wants to generalize beyond the experimental units actually employed in the experiment. Wilk and Kempthorne showed how unit-treatment interactions enter into expectations of mean squares, pointing out that if the number of experimental units in the population of experimental units is large, the bias caused by such interactions will be small.

We have long needed a thoro, authoritative treatment of the Latin square design. Wilk and Kempthorne (80) couched this in relatively easy language and symbolism, presented finite-model expectations of mean squares from which EMS's for the other models can be derived quickly, and concluded that analyses of variance for Latin square designs may overestimate the error term for treatment comparisons and underestimate the component of variance due to treatment main effects. Nevertheless, when both the Latin square and the randomized block designs are reasonable for a proposed experiment, they recommend the former, tho with caution, because its error term for the treatment mean square will usually be too large.

Experimenters owe a debt of gratitude to Scheffé, Cornfield and Tukey, and Wilk and Kempthorne for the care they took to make these articles intelligible to the reader whose mathematics walks with a limp.

Other Articles Concerning Expectations of Mean Squares

In a note (71) and a review (19) Stanley listed and discussed expectations of mean squares for finite, fixed, random, and mixed models, explicitly for two- and three-way crossed classifications with equal numbers of replicates per treatment combination. Extensions to one and to four or more classifications can be made quickly on the basis of principles outlined. Johnson and Stanley (39) exhibited the EMS's for a mixed-model design involving two independent groups of boys, every one of whom responded to 16 projective cards into which were incorporated three dichotomous factors, each level of which was represented twice: $2(2 \times 2 \times 2) = 16$. They showed how randomization at several points in their investigation was essential to the analysis employed. Medley, Mitzel, and Doi (49) provided EMS's for the three-way design without replication tho not via the finite-model EMS's.

Designs

Technical journals, such as *Biometrics*, *Biometrika*, and *Sankhyā*, abound with papers extending old experimental designs and proposing new ones.

The interested reader is referred to these journals and to Cochran and Cox (16). Only a few articles of this type will be cited here.

Zelen (83) explained at a reasonable elementary level his new method for analyzing data from incomplete block designs. Stanley (72) showed that Gellerman's study was more complex than its author supposed, constituting a sort of split-plot crossover design, and explained how to analyze it. Stanley (69) used scores from two forms of a "satisfaction" inventory to compare crossover and noncrossover designs. Pearce (58) studied local versus remote effects of various treatments applied to different parts of the same organism. Morrison (53) illustrated several designs with at least five factors that make possible the testing of all main effects and two-factor interactions while requiring only half the number of observations of an analogous factorial design. Clarke (12) indicated how four 4 x 4 Greco-Latin squares might be used together to provide enough degrees of freedom for error. Stanley (73) dealt with completely permuted 3 x 3 Greco-Latin square designs. Collier and Stunkard (17) treated the same type of design as Stanley and Beeman (74) tho quite differently.

Components of Variance, Pooling Procedures, and Power

Tukey (77, 78) tackled variance components with a new mathematical procedure. Bulmer (10) gave a simple, reasonably accurate formula for the confidence limits of variance components. Searle (65) employed matrix methods to find sampling variances of estimates of components of variance and covariance for a one-way classification with unequal numbers of observations in the various classes. King (40) recommended that in one-way classifications of a random factor, the number of levels of the factor equal the number of observations per level in order to give nearly maximum power for testing the null hypothesis. Johnson's study (38) has already been cited.

Huntsberger (36) showed that a certain weighting procedure provides greater control over disturbances that might result from pooling sums of squares to secure an error term with greater degrees of freedom than does the familiar sometimes-pool method. Bozivich, Bancroft, and Hartley (8) examined critically for some random and mixed models the consequences, with regard to resulting errors of the first and second kind, of certain pooling procedures. They provided two qualified recommendations, concluding that "no rule of the form $V_2/V_1 >$ constant is very satisfactory" (page 1040).

Nicholson's formula (56) for the power of the analysis of variance test holds when the denominator of the F ratio has an even number of degrees of freedom. Fox's extensive charts (29) with a detailed example should be useful. Commins (18) published a table stating the size of sample needed for various assumed values of population parameters and for four probabilities that the study will yield significant results.

A Posteriori Comparisons of Means

Stanley (68) illustrated the "post-mortem" methods of Scheffé, Tukey, Dunnett, and Duncan for comparing various differences among means after an analysis of variance has been performed. Wallace (79) presented Tukey's unpublished procedure, with applications and comments. Kramer (42, 43) extended Duncan's multiple range test to include group means with unequal numbers of replications, adjusted means with heterogeneous variances and covariances, covariance analysis, incomplete block designs, lattices, and other situations.

Applications of the Analysis of Variance to Tests

Hoyt (35) generalized to test items not scored dichotomously his analysis of variance procedure for securing coefficients of equivalence. His result is algebraically equivalent to Cronbach's alpha. Stanley commented on this procedure in his review (19). Moonan (50, 51, 52) showed how to ascertain the equivalence and stability of examinations and the interaction of items with methods in an experiment, using an orthogonal linear transformation due to Nandi.

Nonparametric Approaches to the Analysis of Variance

That current darling of psychologists, nonparametric statistics, is treated in Chapter VI of this issue, except as applied to the analysis of variance.

Roy and Mitra (62: 374) attempted to make a clear distinction between a "variate" and a "way of classification" in order to differentiate between a "multivariate analysis" situation, an "analysis of variance" situation, and "something of a mixed type." Hodges and Lehmann (34) found that the asymptotic Pitman efficiency of the Kruskal-Wallace rank test when compared with the F-test never falls below .864. They then investigated alternative notions of asymptotic efficiency.

Sutcliffe (75) showed how to partition sums of squares and associated degrees of freedom for complex contingency tables of frequency data from multiple classification designs, quite analogously to the analysis of variance. It is interesting to compare this method with the Garner-McGill uncertainty analysis (31) based upon information theory.

McNemar (47) analyzed seven sets of data both by the analysis of variance and Kellogg V. Wilson's method, which is similar to Sutcliffe's but less general, to show that the latter has considerably less power. One might add that with Wilson's partitioning of chi-square the interaction term may be negative.

The upshot of this seems to be that while nonparametric analogues of the analysis of variance are valuable for frequency data, one must be careful not to throw out the baby with the dirty bath water in the interests of simplifying computations.

The Analysis of Covariance

In an important paper, Cox (21) compared covariance analysis with blocking (matching with respect to a covariable) and concluded that methods based upon covariance are preferable to blocking only if the r between the covariate (x) and the dependent variate (y) is at least .6. But if we suspect that the treatment *effects* are not independent of x—that there is a treatment by x interaction—we should ordinarily prefer to use x quantitatively.

Truitt and Smith (76) examined methods for making covariance adjustments in split-plot experiments and testing main effects for significance. We have already mentioned the work of Kramer (42) and Searle (65).

Pairing

Jackson and Fleckenstein (37) compared the Thurstone-Mosteller, Scheffé, Bradley-Terry, and Gulliksen methods for analyzing data based upon paired comparisons, concluding that while all four procedures give about the same results, each has advantages for certain situations.

For both the fixed and the mixed models, Lev and Kinder (44) offered analysis of variance formulas applicable to a group of several subjects observed in the presence of each of the other subjects of the group, the entire set of possible pairings having been repeated on several occasions. Runkel, Smith, and Newcomb (63) presented a method for computing the interaction effects on variables measured by observing interacting pairs of persons, where not all possible pairs of subjects need be observed.

Miscellaneous

In a long, technical article, Box and Hunter (7) continued the development of "Boxism," introducing the concept of the "variance function" for an experimental design and defining "rotable designs." Cochran (13) discussed combining estimates from several experiments and gave examples.

Box and Andersen (6) found that while the analysis of variance test for groups of equal size is both remarkably "robust" (insensitive to extraneous factors not being tested) and "powerful" (sensitive to the specific factors being tested), Bartlett's test for the homogeneity of a set of variances is affected drastically by departures from mesokurtosis. For 20 variances based upon 9 *d.f.* each, Bartlett's test yields a significance level of .718 when kurtosis is 2, instead of the "correct" .05 value! For kurtosis of –1 the corresponding figure is .000004. The authors applied permutation theory to the problem of comparing variances to secure a robust test.

Fisher (26) warned against choosing one transformation of data in preference to another because of computational ease without considering how well it conforms to theoretical considerations.

Smith (66) stepped in to resolve the long-standing discussion in *Biometrics* about whether the missing plot estimate should be considered simply a number to be placed in the empty space or an estimate of the lost observation. He pointed out that the standard error to be attached to the estimate depends upon what one intended *a priori* to estimate.

Articles on graphic methods by Barnes, Pearson, and Reiss (2) and Lyle (46) are well worth perusing.

Concluding Remarks

Many of the contributions to experimental design during the past three years should be incorporated rapidly into statistics textbooks designed for students in education and psychology. Authors of such books need the ability and willingness to translate into simpler but still accurate form relevant material published by mathematical statisticians. Then by studying for at least a year, and preferably longer, under a well-qualified instructor, graduate students may come to understand the rudiments of experimental design. To do less than this and still hope for properly designed experiments is asking for a miracle.

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CHAPTER IV

Research Methods: Status Studies and Sample Surveys

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The many technical articles published in recent years on the theory and methodology of sample surveys and the rise in the number of status studies demonstrate the need for a separate chapter on these subjects. In this connection, it is appropriate to mention that Cornell (16) also prepared a separate chapter on this increasingly important area in a previous REVIEW. Altho, as indicated in the earlier chapter, the theory and practice of sample surveys developed in other fields, education will obviously be one of the biggest users of the method as a research tool. For example, the recent large foundation grants for research in almost all aspects of education usually demand status studies to determine the present position of education in our culture and so provide a basis for comparison and future evaluation.

The major impetus for status studies, and the consequent attention to technics of sample surveys, has been and still is the need for vital statistics which can be used for social research and the determination of public policy. The report on the evaluation of Salk polio vaccine (96), the earlier Kinsey report, and the ever current election straw polls have made the general newspaper-reading public aware of sample survey technics and have indicated the growing need for more statistical sophistication in students in education and social research. It is interesting to note that the sampling design used in the 1954 field trial evaluation of Salk polio vaccine contained flaws similar to those reported by Student (76) in his assessment of the sampling design for the Lanarkshire milk experiment of 1930 (a large-scale status study to contrast the effects of raw milk and pasteurized milk on the heights and weights of elementary-school children in Scotland). An evaluation of the design of the polio vaccine field trials was presented by Brownlee (9), and sampling problems in the Kinsey reports were discussed (37). A rather recent use of the results of sample surveys has been their introduction as legal evidence in judicial decisions. Deming (21) discussed some of the problems involved in these situations.

For purposes of exposition, status studies can be artificially classified into two divisions according to goals. One goal can be illustrated by the operations of United States Government agencies in doing status studies on many national social and economic characteristics for the purpose of collecting and publishing vital statistics. The results of these status studies are then available for government, industry, labor, educational groups, and others to be used as aids in policy making. The fact that some of these studies have become routine operations in no way indicates that the associated sampling design and nonsampling problems have been conquered. A second purpose of status studies is best illustrated by the aforementioned

Salk vaccine evaluation trials. Here a specific goal is in mind before the experimentation begins, and the design is prepared for this one effort.

No matter which goal prevails, the methodological problems are basically the same. Usually some nonsampling restrictions to fit a specific situation are placed on a random sampling mechanism. The most elementary situation of this type is known as stratified random sampling. Other designs, such as systematic sampling, cluster sampling, and multistage sampling, are now described and developed in several texts. Some of these texts by Sukhatme (78) and Hyman (33) have appeared. Chapters on sample design and analysis can also be found in compendiums of technics for social research such as that by Young (100).

A number of papers on theoretical design and assessment have been published and are discussed below. Much of the emphasis in these papers has been on the preparation of a sampling design which minimizes the variability of an estimate, or the cost of a survey, or both. In addition, papers concerned with sampling problems in "dynamic studies," that is, measurements of a trait over time, and papers concerned with measurement problems related to responses to questionnaires and inventories, are beginning to appear. In the main, the present chapter reports on papers published since the middle of 1954, but it also contains some earlier articles of interest.

Status Studies

The U. S. Bureau of the Census undoubtedly leads the way in the application of sample survey technics to status studies in education. Four regularly reported measurements of interest to educational researchers are school enrolment (88, 89, 90, 91, 92), employment of students (83, 84, 85), summary of government finances (93, 94, 95), and school districts in the United States (86, 87). As one can notice and expect, these status studies represent the collection of vital statistics rather than single studies motivated by specific educational problems.

The use of sample surveys to explore specific educational issues is increasing. Holland (31) made a survey of approximately 700 freshmen and sophomores at Michigan State College to assess their perceptions of the instructor. A stratified random sample of approximately 1000 eighth-graders in Kansas schools was collected by Zack (101) to determine the influence of socio-cultural characteristics on educational opportunities in public-school instrumental music. Another stratified random sample of 87 fifth-grade teachers from public elementary schools in Minnesota was used by Johnston (34) to determine the achievement of objectives of elementary-school science. A survey to obtain information for teaching materials for food and nutrition classes was made by Thrift and Ward (80). Interest in the impact of social stratification on occupational expectations of twelfth-grade Michigan boys led Youmans (99) to a sample of 6800 youths from 56 public and private high schools. Hunter (32) studied the attitudes of public-school teachers in a large Southern city toward school and living

conditions thru a questionnaire submitted to a population of about 2000. Tumin (82) used a stratified sampling design involving cluster sampling to study the effect of the exposure to mass mediums of communication on readiness for desegregation among white males 18 years old and older in Guilford County, North Carolina.

Two studies of a noneducational nature, which are of interest from the standpoint of sampling procedures, were concerned with transportation flow data (74) and the estimation of the Brazilian coffee crop (75).

Theoretical Design and Assessment of Sampling Errors

There is obviously a close relation between the development and assessment of sampling designs and the properties of various estimates calculated from the resulting sample data. Indeed, the efficiency of any sampling plan is usually assessed in terms of the variances of the resulting estimates. For purposes of discussion, however, an attempt has been made to divide the references in the broad area of theoretical design and assessment into two groups depending upon whether the primary emphasis of the paper is on the sampling design or on the proposed method of estimation. It is believed that such a division, altho slightly arbitrary in some instances, will be useful in highlighting recent developments in this area.

Sampling Design

Two expository papers presented the basic notions of population sampling in nonmathematical terms. Slonim (73) covered briefly but clearly the concepts of simple random sampling; he also discussed estimation procedures and sampling and nonsampling errors. Jones (35) discussed the meaning and purpose of sampling, the relative merits of different sampling procedures, and methods of minimizing the costs of random sampling. Both authors drew illustrations from their respective fields, namely, the Air Force and the telephone industry, and the papers should prove generally useful in providing insight into sampling theory and procedure.

Expository discussions of some specific sampling problems were given by Deming (20) and Dalenius (18). Deming presented a simplified procedure for the selection of a sample and for the numerical computation of the standard errors from the returns. Dalenius discussed the various methods proposed for determining sample sizes in stratified random sampling when the survey is designed to provide information on more than one variable.

A general discussion of stratified random sampling was given by Aoyama (1). The author considered, among other things, the selection of controls for stratification, the influence of stratification on the estimate, and a method of analysis of data based on a modification of Tchebycheff's inequality.

In a paper on two-stage sampling, Sen, Anderson, and Finkner (71) reported on an empirical investigation of various stratified two-stage sampling systems for estimating totals of certain agricultural items in North Carolina. The investigation represented the application of theory developed by Sen to the selection of two primary sampling units without replacement from a stratum where one of the units is selected with probability proportional to size and the other with equal probability.

The problem of planning a two-stage sample involving multiple correlated characters was considered by Chakravarti (11). A model was formulated for the problem, and three procedures for determining optimal sample sizes were given, depending upon particular conditions to be optimized. A numerical illustration was also given.

In other papers on two-stage sampling, Brooks (6) considered the estimation of an optimum subsampling number when the ratio of the variances within primary units is not known but must be estimated. Rangagajan (61) compared two methods of selecting second-stage units from primary units, one in which the number of second-stage units is fixed in advance and a second in which the expected number of such units is fixed in advance.

Multistage sampling plans were discussed by Banerjee (2), Basu (3), Cansado (10), Raj (57), and Roy (65).

An extension of present sampling theory to the problem of sampling over time was considered by Eckler (22). The problem of interest is that of estimating the time-dependent mean of a population. In such a case information contained in earlier samples may be used to improve the current estimates, provided the various samples have some elements in common. A plan for sampling over time such that some old elements are eliminated and new elements are added each time a sample is drawn is called rotation sampling. Three methods of rotation sampling were described in the paper and compared on a cost basis.

Among papers on special sampling designs was one by Patterson (54) which compared four methods of selecting a lattice sample. Krishna Iyer and Singh (41) considered distance travelled as one factor in a lattice design. Sen (70) investigated a multivariate sampling design in which successive observations were not independent. Raj (58) studied the selection of two overlapping samples for multipurpose surveys.

Papers by a number of authors dealt with the choice of sample sizes. Yanedo (98) gave a rule for choosing in stratified sampling between sample sizes proportional to stratum sizes and sample sizes proportional to stratum sizes weighted by estimates of the stratum standard deviations. Optimum allocation, that is, the allocation minimizing the variance of the required estimate, for a successive sampling plan involving correlated variables was considered by Tikkiwal (81). Putter (55) considered an optimal linear decision rule for allocating the sample sizes in the second stage of sampling from a stratified normal population. Grundy (29)

discussed a method of stratified sampling with probability exactly proportional to stratum size.

In a comparison of sampling with and without replacement, Kozniewska (40) concluded that sampling without replacement was more efficient for unstratified random sampling. Rios (62) demonstrated that the expected number of distinct elements in sampling with replacement is greater than or equal to the number in sampling without replacement when the variances of the corresponding averages are equated. Singh (72) stated that in a stratified design, the selection of primary units without replacement is usually more efficient when the number of primary units is two, but is not necessarily so in other situations.

In investigating area sampling in agricultural problems, Mokashi (47) compared four types of sampling designs for estimation of timber volume per acre. A cluster sampling plan for estimating crop acreage was also considered by this author (49).

Moser (50) described the principal developments in the sampling of human populations in Great Britain during the past five years, discussing changes in methodology together with new applications. Zarkovic (103) discussed sampling methods in the Yugoslav 1953 population census.

Among the other papers listed, Chapman (12, 13) and Chapman and Junge (14) considered probability models and sampling methods for biological populations which are often mobile in space and difficult of access. Sundrum (79) discussed a method of systematic sampling based on ordered properties. Matthai (46) considered the selection of random numbers for large-scale sampling. Jones (36) discussed the use of random subsample means to evaluate variability and possible bias in samples. Rios (63) considered several problems of maximums and minimums in sampling from a finite population. Kitagawa (39) discussed some problems in survey design.

Methods of Estimation

The problem of estimating the total value of a character in a finite population on the basis of a sample was considered by several writers. Raj (60) proposed unbiased estimates when sampling units are selected with varying probabilities without replacements, and gave exact expressions and unbiased estimates for the corresponding variances. The same author (59) also studied the properties of ratio estimates in sampling with equal and unequal probabilities. Ronge (64) compared ratio and linear estimates of a population total when data on the value of the variable are known for an earlier time.

Das (19) considered the estimation of a population total and its variance in a finite population when the estimate was based either on a specified type of two-stage sampling or on sampling with varying probabilities. Sen (68) also discussed the estimation of the variance in a finite population.

More general types of linear estimates were studied by Godambe (27) and Raj (56). Three papers of Masuyama (42, 44, 45) concerned estimating a total area on the basis of areal samples.

A general procedure for constructing unbiased estimates of the mean value of a variate in a finite population for a specified two-stage sampling design was given by Sandelius (67). A procedure was also given for providing unbiased estimates of the variances of the estimates. A minimum variance unbiased estimate of the mean of a given group when samples are also available from correlated variables in other groups, was constructed by Narain (52). Properties of a sample mean were studied by Bennett (5).

Studies of the variance of estimates were made for ratio estimates in stratified sampling by Mokashi (48), for unbiased estimates in cluster sampling by Yamamoto (97), and for Gini's mean difference in samples from a finite population by Salvemini (66). An outline of a general theory for estimating variability among strata for a specified sampling scheme was given by Sen (69).

Technics for estimating the intercensal population of counties were given by Brown (7) and Crosetti and Schmitt (17). A method of adjusting census estimates to agree with other available census data was discussed by El-Badry and Stephan (24).

Nonsampling Errors

Initial emphasis in sample survey technics was on sampling designs which would yield unbiased estimates of the desired population values. These designs, over the years, have been refined and elaborated to take advantage of possible patterns of variability in the population. Most of these designs, however, began with the assumption that once an individual was selected for the sample, the desired information was obtainable in an accurate and reliable form.

It was early recognized, however, that problems of nonresponse and accuracy of reported data existed. In fact, in discussions of the usefulness of sample surveys as opposed to complete enumeration, an argument frequently advanced was that estimates based on sample data collected by a small number of trained workers were preferable to counts based on complete enumeration carried out by a large number of untrained people.

Discussions of nonsampling errors are now appearing more frequently in the literature. Sukhatme (77), for example, discussed the measurement of observational errors in surveys. A sampling procedure to deal with the problem of nonresponse in mailed questionnaires was developed by El-Badry (23). The effects of nonresponse were also considered by Brownlee (8) and Cohen and Lipstein (15).

In some instances, the accuracy of reported information is checked by taking a second sample from the original data and collecting more detailed information for the individuals in the second sample. Thus, Kish and Lansing (38) reported on discrepancies in the estimates of market values

of homes made by the home owners and by professional appraisers. Zarkovic (102) discussed the use of sampling methods to evaluate the accuracy of literacy data. The validation of morbidity survey data by comparison with hospital records was studied by Belloc (4).

The effect of memory on reported responses was investigated by Gray (28) using data from the British Survey of Sickness. An attempt to measure errors due to editing of questionnaires in a census was made by Nordbotten (53). The effect of ignorance on opinions of economic and social issues was considered by Ferber (25). Ferber (26) also investigated the consistency of replies of various family members. The error in crop-cutting experiments due to the bias on the border of the grid was discussed by Masuyama (43).

Hansen and others (30) reported on a redesign of the Current Population Survey to provide for a more efficient system of field organization and supervision as well as on some advances in methods.

A study by Myers (51) of the accuracy of age reporting in the United States concluded that accuracy has improved over the past 70 years particularly for native-born white males. He stated, however, that there is need for improvement in the nonwhite population, and that in general, reporting of ages by women is significantly less accurate than such reporting by men.

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CHAPTER V

Research Tools: Library Resources

LOLA R. PIERSTORFF

THIS description of library resources and bibliographical technics brings up to date the similar chapter by Good (14) in the December 1951 issue of the REVIEW, covering the materials published since June 1951. The topics treated include (a) library services, manuals, and general aids; (b) guides to books and periodical literature; (c) guides to theses and selected research projects; (d) serial and occasional bibliographies and summaries; and (e) institutional directories or handbooks.*

Library Services, Manuals, and General Aids

Brickman (5) again reviewed the chief reference works in education. Barton (3) prepared another revision of her brief guide to reference books. Shores (33) gave a detailed discussion of basic reference sources with sections on references by type and subjectmatter area. Two supplements to Winchell and Johnson's bibliographic guide to general reference books (44) were completed.

A research methods bibliography has been a part of *Research Studies in Education* (7) since the 1953 issue. Seeger (32) gave very practical suggestions on the use of library resources in educational research.

Kinney (20) produced a guide to bibliographical style manuals and their use in documentation and research. Campbell (9), Dugdale (12), and Turabian (38) revised their manuals for writers; these publications deal chiefly with style. McCrum and Jones (24) wrote a manual devoted to bibliographical procedures and style used in the Library of Congress.

Guides to Books and Periodical Literature

The guides to books included the annual selection of outstanding educational books (10, 29) and books in education considered significant for an eight-year period (45).

Educational Measurement

The *Fourth Mental Measurements Yearbook* (8) remained the most nearly complete listing of tests and their evaluations; it also listed and reviewed books in this field. It covered the period 1948 thru 1951.

* A mimeographed bibliography of references additional to those given in this chapter is available from the author while the supply lasts.

Psychology

Six more volumes of the *Annual Review of Psychology* (1) were issued. While the contents varied slightly from volume to volume, there regularly were reviews of areas interesting to educational workers, for example, learning, counseling, statistical research and design, educational psychology, and child psychology. A new book-review journal (11) with author, reviewer, title, and subject index of selected books was started. Latham (22) reviewed guides to literature in psychology.

Textbooks

Textbooks in Print (35), formerly *American Educational Catalog,* is a compilation of all textbooks, with excellent indexes by author, title, and subject.

Periodicals

Tangible evidence of the importance of periodical literature was provided in a study by Saunders (31) for UNESCO. For example, he found that the total number of journals referred to in the *Encyclopedia of Educational Research* approached 400. He concluded that the indexing of periodical literature in the social sciences and humanities was not as satisfactory as that in the natural sciences and that altho education was one of the best served social sciences, there was considerable room for improvement. Another UNESCO publication (39) listed educational research journals in 44 countries.

Guides to Theses and Selected Research Projects

Beginning in 1956, the Trotier-Harman yearly index of doctoral dissertations (37) was consolidated with *Dissertation Abstracts* (26). The latter then became the standard annual comprehensive list of doctor's dissertations.

Periodic compilations of titles of dissertations, theses, reports, and field studies in education were begun. Blackwell (4) listed research in education and educational psychology presented for higher degrees in the United Kingdom and Ireland beginning in 1918. Brown, Lyda, and Good (7) began an annual listing of doctoral dissertations completed and under way in education together with a research methods bibliography for the year. The first section, altho possibly more helpfully arranged, substantially duplicated Trotier and Harman (37) and later, the corresponding section of *Dissertation Abstracts.* The second section continued the reports originated in the *Phi Delta Kappan* (17); Good's bibliographies

are helpful. Lamke and Silvey (21) began an annual classified listing of titles of master's theses in education presented for degrees in the United States and Canada beginning in 1951-52. The compilers expressed the hope that making such research generally known might help give it stature.

Indexes and abstracts of foreign physical education literature were made available beginning in 1955 (18). The Clearing House for Research in Child Life changed to semiannual publication (42) and no longer lists certain kinds of medical studies. UNESCO (39) listed educational research bibliographies and directories in 44 countries. Bibliographies of dissertations in fields related to education are easily located in the well-known *Education Index;* therefore they will not be listed here.

Serial and Occasional Bibliographies and Summaries

Other continuing bibliographies or summaries included guidance (16), occupational literature (13), audio-visual education (23), history of education (6, 19, 43), psychiatric books (25), reading (15, 36), teacher education (2), articles on education in lay magazines (27), criticisms of education (34), and questionnaire studies in education (28).

Institutional Directories

New education directories or handbooks were started on the international (30, 40, 41) and the national (46) levels.

Summary

This chapter has been devoted to technics and procedures for work with reference tools in the library. Research theory has always been 10 to 20 years ahead of practice for many reasons. The development of action research, study councils, a variety of new reference tools, and summaries of research have been particularly valuable in stimulating application.

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CHAPTER VI

Research Tools: Observing and Recording Group Behavior

MARVIN TAYLOR and HAROLD E. MITZEL

During the period covered by this Review, discussion and experimental activity concerned with group behavior continued at a vigorous rate. Altho the years cannot be characterized as encompassing great originality in methodological construction, it seems safe to argue that they have been dominated by continuous refinements of useful observational methods and by increased knowledge of variables which act to bias and distort the observations themselves. A noticeable characteristic has been the continued use of observational and recording technics for such practical purposes as assessment and selection programs.

This review of the literature represents only a sample of the numerous research studies in the area. The authors have tried to limit their study to experimentation which more or less directly involves in its design the exploration and/or refinement of technics of observing and recording group behavior. It should also be noted that the total literature on this topic is not represented because of the exclusion of studies reported in languages other than English. The authors regret their lack of access to material from this expanding source.

Measurement of Group-Membership Interaction

During the period under examination, energy was invested in locating and studying variables which affect group-member interaction. Jensen (30) suggested a seven-faceted conceptual framework for observing both the social structure and the interaction within classroom groups. The dimensions for study were (a) problem solving, (b) authority-leadership, (c) power, (d) friendship, (e) personal prestige, (f) sex, and (g) privileges. The remainder of this review will deal with the various approaches to these areas.

At Ohio State University a 10-year period of interdisciplinary research was described in a series of monographs. One report in this series was by Stodgill and Shartle (51) who studied administrative and leadership relationships within an established, hierarchal organization. They described the development of an interview technic which yields sociometric-type data, and a set of scales to measure the leader's perception of his responsibilities, authority, and delegations (*RAD* scales). Several other technics which were developed will be discussed in a later section. In a second monograph, Fleishman and others (17) produced statistics relevant to a scale called the *Leadership Behavior Description Questionnaire* (*LBDQ*). This scale was used in an industrial setting by foremen and

their subordinates and was useful in predicting and describing the relationship between these individuals. Halpin (22) described the *LBDQ* as a two-dimensional scale, containing 40 items selected from a factor-analysis matrix. It was designed to measure the behavior of a leader in terms of how he "initiates structure in interaction" and how much "consideration" he exhibits. Halpin further pointed out that the scale had been used fruitfully in measuring the effectiveness of leaders in military, industrial, and educational settings. Hemphill (27), at one time associated with this project, utilized five or six scales in a study of the relationship between behavior of college department heads and the reputation of the departments for being well administered. In addition to the *RAD* and *LBDQ* scales already described, of particular interest is the *Group Dimensions Description Questionnaire.* In his manual Hemphill (26) stated that this instrument was designed to elicit a respondent's perceptions, attitudes, and feelings about his group. By combining every group member's scores it was also possible to obtain a profile of the major dimensions which characterized the group as seen by its members. Hemphill stated that the scale samples 13 dimensions of group structure and reported data to support claims for reliability and validity.

Shevitz (48) investigated Hemphill's hypotheses regarding leadership origins in three-man groups by obtaining data from two trained observers. The observers utilized behavior categorized according to a scheme worked out by Hemphill and the Ohio State group. Olmsted (44) developed a 22-item fixed-response questionnaire to evaluate the adequacy of leadership of the formal group leader. Of particular interest may be the measure of "leader favorableness" which was claimed to be independent of the usual halo effect found in such measures.

A more recent development was the emphasis on peer- and self-ratings. Webb (59) studied the relationship between self-ratings and objective measures of intelligence. The rating device used was very simple, requiring subjects to rank their peers and themselves into a normalized scale from most to least intelligent. The results prompted Webb (58) to develop a novel form of self-group ratings which would yield high reliabilities for self-measures. In this technic, "Self-plus-minus," the individual compares himself with every other member of his group on a particular trait. The result is a greater number of self-ratings and higher reliabilities. Mayo (38) studied the relationship between peer-ratings and halo effect and reported that not all of the variance found was attributable to halo effect. Suci, Vallance, and Glickman (52) found that peer-rating reliabilities were not markedly affected by variation in the objective basis of the choice, or by the rater's liking certain members of his group. Hoffman and Rohrer (28) developed a peer-evaluation scale. The score on this scale may be generalized to groups outside the reference group from which the score was obtained. Buchheimer and Pendleton (7) studied the *Group Participation Scale* originally devised by Pepinsky, Siegel, and Van Atta and tentatively concluded that it is reliable and valid. On this instrument,

group participants rate each other on such behaviors as "*initiate, sustain, define*, and *direct* activity toward goals which were held by the group." In summary, the work in this area is promising and encouraging. The technic appears to be quite useful for obtaining intimate data about intragroup relations which are not easily accessible to the observer's eye or to other forms of paper-and-pencil tests.

Sociometric Instruments

An area of considerable activity lies in the development and refinement of sociometric-type instruments. Marshall (35) reviewed previous attempts to study the relationship between sociometric choices of preschool-age children and several criteria of social behavior. She concluded that the use of antiquated statistical procedures and methods of investigation had not yielded fruitful results. Acting on this conviction, McCandless and Marshall (33) constructed a picture sociometric test for use with preschool-age children, utilizing large photographs of children in the same group and several oral sociometric-type questions. In a later study (36) they investigated the relationship between choices of friends and such variables (observed by a group of sophisticated judges in two-minute segments) as associative play, friendly approach, conversation, hostile interaction, attention, and no response. The outcomes suggest that this may be a valuable technic.

In an approach distinguished by its originality, Gardner and Thompson (19) described the development of five social relations instruments. In constructing their scales, which yield near normal distributions, the authors tried to take into account such factors as (a) the ambiguity of needs underlying the choice, (b) the inequality of rankings by means of a nominations approach, and (c) the lack of generalizability of data from one group to another. By manipulating the obtained data, eight indexes of an individual's social relations status in a group and nine indexes of social group structure are calculable.

Not all the activity in this field was in the development of new instruments. Mouton, Blake, and Fruchter (42, 43) analyzed 53 studies conducted in military, industrial, and educational settings and concluded that the sociometric-type test has considerable reliability and validity. They also suggested some of the variables which affect data and commented on additional uses for the obtained information. Davitz (14) studied the relations between sociometric choice and perceived similarity and dissimilarity. Of particular interest to other investigators should be the manipulation of sociometric instruments so as to measure perceptions of similarity and dissimilarity. Vidich and Shapiro (56) in a study using a large sample found that measures of prestige as revealed by a sociometric-type questionnaire and an anthropological field-worker's ratings were complementary but not overlapping.

The number of choices to be allotted a subject, another area of concern in the construction of sociometric tests, was investigated by Gronlund (20). He was interested in the stability of weighted and unweighted scores based on three, four, or five choices. Tagiuri, Bruner, and Kogan (53) developed a mathematical model for computing the chance frequency and variance of the dyadic relationship obtained within relational analysis. Keislar (31) constructed a special scoring formula for the "Guess-Who" type questionnaire which is claimed to be more valid than and just as reliable as "older" technics. The special advantages of the "new" formula lie in the acquisition of a normal distribution and the minimizing of the effect of unequal familiarity of all members in a large group.

Barr (4), Luebke (32), and Hale (21) discussed ways of analyzing and charting or mapping the results of sociometric tests. Hale's particular contribution was the development of five criteria, derived from current group dynamics literature, which might be used to measure the social growth of a group over a period of time as revealed by test-retest sociometric data.

Direct Observational Technics

A more time-consuming technic for studying group-member interaction is the direct observation of a group in some artificial or natural setting. This form of obtaining information requires large amounts of time because of the training of judges or raters and because of the number of raters used being usually larger than is the case with sociometric or peer-group ratings. As one might expect, however, direct observation is particularly effective in investigations of certain kinds, for example, communications, problem solving, and the like. Bales (2) designed a 12-category interaction scale which allowed the trained observer to capture the flow of interactions within a group. Altho the original scale was completed prior to 1954, the article reviewed is particularly useful for the novice or the lay individual interested in dynamics of small-group interaction. Withall (62) described a technic for obtaining a measure of the teacher's classroom interactions which eliminated the use of many judges, but on the other hand, probably entailed a large expenditure of money for equipment. Altho no evidence was presented to show that the technic used was reliable or valid, the use of a time-lapse camera clicking pictures every 15 seconds and a sound tape recording must be considered an ingenious way to obtain information about teacher-pupil interactions. Moustakas, Sigel, and Schalock (41) developed an observation schedule which has 82 units for describing child behavior and 89 units for describing adult behavior. The categories were designed to measure overt behavior and none was constructed to be interpretive or evaluative. The authors claimed that observer and category reliability were high.

Most of the experimentation in group dynamics is usually designed to include some combination of observational and paper-and-pencil technics. The concluding pieces of research to be reviewed will be considered according to the problem area they represent.

Communications

The most frequently utilized technic in the study of communications is direct observation. Hearn (24) used the Bales categories to determine the direction of remarks between members of the group in leaderless and in trainer-dominated sessions. Porter (47) studied the relationship between the type of participation in a small-group discussion, as measured by the Bales categories, and feelings of satisfaction. Cervin (10) predicted from a stimulus-response-type model that a person of high emotional responsiveness would speak first, participate more, and change his opinions less than a person of low emotional responsiveness. Emotional responsiveness, as measured on a Guttman type paper-and-pencil technic, and observation of the group supported the original predictions.

Group Problem Solving

The comparison of the effectiveness of group versus individual problem solving, the development of criteria for measuring effectiveness, and the process of group problem solving were of considerable interest to investigators. Dickens (15) devised a formula based on the hypothesis that one aspect of effectiveness is the spread of participation among group members. McCurdy and Eber (34) defined effectiveness on a three-man light-switch series task in such behavioral terms as (a) time per unit of work, (b) correct switch turnings per unit of work, (c) errors per unit of work, and (d) errors per unit of time. These criteria seem to be a successful attempt to locate facets of a situation which may be objectively observed and recorded. Torrance (55) had members of bomber crews respond to projective-type pictures construed to be "psyche-group" and "socio-group" oriented. He studied the relationship between a group's perceptions of its functioning and its actual performance according to military criteria. Fiedler (16) studied the relationship between a basketball team's effectiveness and the assumption of similarity among its members. The measure of assumed similarity was derived by analyzing the responses that the team members made for each other on a questionnaire. The merit of these methods apparently lies in the depth of analysis they permit the investigator.

Hays and Bush (23) employed a mathematical model to predict group decision making. Their models took into account two types of group action: the group-actor model where individuals decide together, and the voter model where the individuals' choices are independent and the decision is by majority vote. Damrin (13) developed a unique problem-solving test

under the sponsorship of the Russell Sage Foundation. This test involved the use of a set of 36 interlocking blocks in different colors. Each member of the group was given one or two blocks and the group was then instructed to make a plan for reproducing a model figure constructed from another set of blocks shown them by the test administrator. The test was successfully used from the third-grade level to adult groups, and wide variations in the quality of group performances were noted at every level. Methodological problems connected with the technic include (a) quantification of group performance from observers' protocols, (b) reduction of bias introduced by personality of test administrator, and (c) the effects of group size on performance. The *Russell Sage Social Relations Test* will undoubtedly be refined and modified to become an important measure of group performance. Even without these refinements in its present stage of development the *RSSR Test* provides valuable demonstration material for workshops interested in cooperative educational goals.

Conformity

An area of increasing concern to social psychologists is conformity behavior. Matthews and Bendig (37) formulated a statistic, the "Index of Agreement," which quantifies the amount of agreement between group discussants. Simon and Guetzkow (49) devised a mathematical model to examine Festinger's five hypotheses relevant to group pressure, communications, and movement toward uniformity of opinion. Five equations were developed which take into account a correction for time and a variable designated as feedback. This particular construct is very useful in explaining much of the variance in Festinger's and in his colleagues' experiments. Blake and Brehm (6) in a novel project demonstrated how a tape recording of a simulated group could produce conformity on the autokinetic perceptions of an uninitiated group member. Burdick (8) also produced conformance of opinion about juvenile delinquency by using a simulated group discussion. The methodology of these two studies and their results seem to suggest a very economical way to study group pressure effects and perhaps other group effects upon individual behavior. Crutchfield (12) described a laboratory-type arrangement for studying conformity behavior. His "quasi group-interaction method" serves to standardize the situation; hence the stimuli for each subject are identical.

Social-Emotional Climate

Forlano and Wrightstone (18) revised the *Ohio Social Acceptance Scale,* a peer-rating instrument, to assess the quality of social acceptance in a classroom. Social acceptance was operationally defined as the percentage difference between the median acceptance ratings and rejection ratings within a class. The results of their study suggest a marked use for this measure. Wandt and Ostreicher (57) devised 14 scales, each containing a

nine-step interval to measure the consistency of teacher behavior in the area of social-emotional climate. Gardner and Thompson (19) developed several technics for measuring "esprit de corps" and "group effectiveness" components of morale.

Phillips and D'Amico (46) purported to measure group cohesiveness by observing changes in response on a pre-post sociometric-type test. The resulting changes were analyzed to study the relationship between cohesiveness and the social-emotional climate the group was required to work within while solving a problem. Pepitone and Kleiner (4) formed groups of male campers into teams on the basis of sociometric choices and observed the changes in cohesiveness as the result of varying the probability of status gain or loss.

Role Behavior

Many of the studies of role behavior utilized a combination of direct observational technics and subject self-ratings or group-ranking procedures. Talland (54) used the Bales Interaction categories and a therapeutic interaction analysis scale to study the structuring over a period of time of 15 initially informal psychotherapy groups. The subjects also ranked their fellow group members on a scale designed to measure status perception. Slater (50) likewise used the Bales categories to study role differentiation as affected by the manipulation into high-status groups (members perceive themselves as similar in problem-solving effectiveness) and low-status groups (members perceive themselves as dissimilar). Mitchell (39) observed six sessions of a citizens committee seeking to evaluate itself. The roles played by the various members were categorized as problem-solving behavior, maintenance of group atmosphere, and "group blocker" behavior.

Another method for obtaining information relevant to the role an individual and/or his group perceive him playing is to request responses from the group and the individual on such devices as sociometric tests and peer-self ratings. Crowell, Katcher, and Miyamoto (11) designed two questionnaires dealing with a person's feelings about his skills as a communicator and a communicant. Subsequently, self- and group-member-ratings of performance in three discussion groups were related to the self-perceptions of the individuals. By manipulating the instructions on the same sociometric-type instrument, Hollander and Webb (29) were able to study the relationship between leadership, followership, and friendship among a group of naval air cadets. Carter (9), as the result of a careful factor analysis of a great many previous studies, found three factors which might be useful in the study of leadership behavior. The three which emerged from the study were (a) achieving various personal goals, (b) aiding attainments by the group, and (c) sociability.

Assessment and Selection

Ever since the OSS assessment program demonstrated the feasibility of using group technics in selecting men for critical jobs, other organizations have attempted to use these technics in the selecting and training of candidates for vital positions. Perhaps the major methodological problem for the assessment team is the development of situational tests which yield data relevant to prediction of individual performance on a job. Weislogel and Schwarz (60) suggested three criteria for designing an effective problem. Crutchfield (12), in dealing with the problem of measuring conformity to group judgment, suggested criteria which should be accounted for in the construction of any type of situational test. Bringing their experience to bear, Bales and Flanders (3) discussed the planning necessary for setting up a functional observation room for studying small groups. They treated such topics as comfort and space requirements, needs of research staff, visiting spectators, and the design of the experimental room.

Bass (5) studied the utility of leaderless-group discussions for evaluating leadership behavior and concluded that the technic had high validity as well as predictive value. Wilson and Robbins (61) reported the use of leaderless-group discussions as one aspect of a selection program for potential guidance workers. This article dealt particularly with such problems as the size of the group being observed, the number of judges, the effect of judges' presence, and the length of the discussion period. In considering the usefulness of these technics, one must also be concerned with the handling of the accumulated information. Morris (40) studied some of the factors which affect the validity of the judgment of assessors and concluded that a consensus of judges' ratings may result in more valid judgments. He further asserted that one must analyze the grouping factors which may influence the judges' consensus. Altho this technic takes more time, both in terms of testing hours and rater time, one can only hope that other organizations will begin to find this technic promising enough to experiment with it in the selection and assessment of candidates for various positions.

Conclusion

The prognosis for future improvements and refinements in the methodology of observing and recording behavior in groups seems good. The period of this review is highlighted by the replications of previous studies, the development of mathematical models, an increase in interdisciplinary research as exemplified by the Ohio State studies and the December 1954 issue of the *American Sociological Review* (1), and the expenditure of large amounts of money for long-range studies in natural settings, for example, military and industry.

What must follow in the coming years if group theory and methodology are to advance is (a) the continuous development of more rigor-

ous technics of locating and measuring variables connected with group characteristics and group structure, (b) a closer relationship between theory and data gathering (too many studies appear to gather data and fit a theory to them rather than vice versa), and (c) a greater uniformity in semantics. Hemphill (25) called for the development of a taxonomy of group characteristics which would operationally define its terms as well as permit accurate measurement. Altho these objectives may seem idealistic, it is unlikely that advancements in group theory or methodology can continue without some attempt to approach them.

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CHAPTER VII

Research Tools: Scaling and Measurement Theory

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THIS CHAPTER covers scaling theory from June 1954 to June 1957. Limited space precluded several pertinent references, and those included reflect an attempt to give adequate coverage to developments deemed important by the reviewers. Elementary texts on scaling methods by Guilford (35) and Edwards (25) appeared during this period.

Theories of Measurement

Old controversies about fundamental measurement in psychology were recently revived. Siegel (70) believed that arithmetic operations and statistical technics based upon them were limited to scales with interval properties, since "the operations allowable on a given set of scores are dependent on the level of measurement achieved." Stevens (72) concurred, but Savage (66) disagreed. Mount (61) denied the traditional necessity of demonstrating physical manipulations paralleling various scale operations and believed numerical measurement to be justified merely by the specification of instructions for assigning objects or attributes to a numerical reference system.

The recent appearance of a strong trend toward axiomatization in scaling theory will undoubtedly contribute to a resolution of such controversies. Suppes (75), starting from a set-theoretical definition of a theory of measurement, formalized the logical foundations of scaling and provided an axiomatic treatment of models underlying ordinal, interval, and ratio scales. Scott and Suppes (67) also discussed logical aspects of measurement, particularly the conservation of axioms. Coombs (17) meanwhile clarified some properties of underlying models by classifying interrelationships between the scaling of individuals and the scaling of stimuli. In another classificatory scheme, Coombs (16) located known scaling methods in a 2 x 2 x 2 table.

Axiomatic Models

Several axiomatic models for different types of measurement are considered together in this section solely for convenience in referring to a unified approach to model construction. Davidson and Suppes (20) axiomatized utility and subjective probability in terms of an indifference relation for alternatives equally spaced in utility. Adams and Fagot (3) intensively analyzed a two-dimensional measurement model assuming

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additive utilities. Luce (49) formulated a probabilistic theory of utility, and a theory of utility discrimination in terms of "semi-orders" (50). Semi-orders do not imply that the relation of transitivity holds; that is, $a=b$ and $b=c$ do not necessarily imply $a=c$. A simplified axiomatization of semi-orders was given by Scott and Suppes (67). Galanter's attempt (29) to apply experimentally an intransitive matching relation serves to illustrate the difficulties currently encountered by many axiomatic models in handling the concept of error.

Luce (51), in perhaps the most important paper reviewed in this chapter, formulated a probabilistic theory of individual choice behavior based upon an intuitively reasonable axiom with extremely general and powerful consequences. The basic axiom may be stated as follows: If T is a finite set of elements and R is a subset of S, which is a subset of T, the probability $P(R;T)$ that an individual will select an element contained in the subset R when the choice is restricted to T is equal to the probability that the chosen element is in R when selected from S times the probability that it is in S when chosen from T; that is, if T is finite and $R \subset S \subset T$, then $P(R;T) = P(R;S)P(S;T)$. One important consequence of this axiom is the existence of a ratio scale v, which may be determined in several ways from different probability combinations. The relation between paired-comparison probabilities, $P(x,y)$, and the underlying scale values $v(x)$ is as follows: $P(x,y) = \dfrac{v(x)}{v(x)+v(y)}$.

Decision Process and Utility Measurement

Davidson, Suppes, and Siegel (21) tested their axiomatic model of utility (20) in a betting situation. Subjects were confronted with choices between two options: one offering a 50-50 chance of winning a or losing b and another offering a 50-50 chance of winning c or losing d. Preferences between options were used to order utility differences among elements. Considering the fact that these models make little provision for handling error, the goodness of fit obtained is very encouraging. Siegel (69) also used preference between probability combinations of stimuli to order scale intervals. Such options as a 50-50 chance of winning a or c versus certainly receiving b were used to generate a "higher-ordered metric scale." Hurst and Siegel (42) applied this scaling technic by betting with prison inmates for cigarettes. They reported that utility was not linear with number of cigarettes. Their treatment of scaling errors was vague. Davidson and Marschak (19) presented experimental evidence that choice behavior may be considered "stochastically transitive." Edwards (26) employed experimentally an additive hypothesis (3) that the utility of n identical bets is n times the utility of one such bet.

The use of bets in decision studies represents an interesting gimmick for obtaining metric information about utility intervals while avoiding the

interaction effects that may plague methods using composite stimuli. At first glance, these betting procedures seem of dubious value in psychological contexts. On the other hand, traditional scaling methods may have overlooked the role of subjective probability in individual choice behavior. Responses to questionnaire items may perhaps be fruitfully regarded as decisions under uncertainty—uncertainty about circumstances in which an item is meant to apply, about the use to which the responses will be put, and the like.

Coombs's Models and Methods

Coombs's scaling models have been criticized for their inability to handle error. Tho Coombs did not come forth with a full-dress theory of error, a tendency to meet this problem can be observed. Coombs (15) presented an assortment of judgmental methods for scaling stimulus similarity, and incidentally recommended replication of judgments as a means of catching occasional errors. Dember (22) applied one of these technics to the scaling of gray patches differing in brightness. He suggested the use of differential reaction times to resolve inconsistent judgments.

Runkel (65) applied unfolding technic as an indicator of interpersonal "cognitive similarity." Coombs (18), in discussing the construction of a "social utility scale," was able to apply the unidimensional unfolding technic. In this treatment he often wrote as tho an interval scale allowing continuous distributions was uppermost in his mind. Thus it would appear that Coombs has budged slightly from the extreme position sometimes imputed to him.

Paired Comparison Models

Morrissey (60) and Gulliksen (37) independently presented equivalent least squares solutions for incomplete paired comparisons. The latter suggested an iterative procedure which markedly reduces computation time. Gulliksen and Tukey (39) proposed a variance-components analysis for the reliability of paired comparisons. Gulliksen (36) utilized a quantity called the "comparatal dispersion" ($\sqrt{\sigma_i^2+\sigma_j^2-2r_{ij}\sigma_i\sigma_j}$) to measure accuracy of paired comparison judgments. Harris (40) revised Thurstone's Law of Comparative Judgment to include asymmetries between a comparison pair AB and its experimentally independent complement BA. The revision permits the extraction of time error and order effects. Gulliksen (38) tested four laws for predicting the scale value of a composite stimulus from the scale values of its components. Linear and negative exponential curves both gave a good fit for food preferences. Rimoldi (62) also obtained a linear relationship for predicting scale values of combined stimuli from components in a study of "famous men you'd like to know." In this way the Thurstone methods, thru the scaling of composite as well as single stimuli, offer an alternative to bets in the measurement of utility (21).

Kendall (45) derived scale scores by finding the principal characteristic vector of a paired-comparisons matrix. The technic is applicable to incomplete data and may be used on a single set of judgments from one individual. Suppes (75) gave an axiomatic formulation of a stochastic model for paired comparisons which assumes that *P(a,b)*, the proportion of times *a* is preferred to *b*, is a monotone increasing function of the difference in scale values; that is, $P(a,b) > P(c,d)$ implies $v(a) - v(b) > v(c) - v(d)$. Suppes described a linear programing solution to these inequalities.

In the Bradley-Terry model (11, 34) for paired comparisons, *P(a,b)* is related to scale parameters π by $P(a,b) = \dfrac{\pi a}{\pi a + \pi b}$. This relationship also appeared in the Luce model (51) discussed above. Thus the "seemingly arbitrary" (47) Bradley-Terry formulation is by hindsight justified thru Luce's axiom. Furthermore, a powerful means for applying the Luce model experimentally is thru the extensive Bradley-Terry machinery (12). Abelson and Bradley (2) applied the method to stimuli with combined attributes in a 2 x 2 factorial. Such an application suggests a multidimensional extension of the model for known stimulus dimensions. It is of interest that if the logistic curve is substituted for the normal ogive in the Thurstone model, log πa corresponds to Thurstone's scale value (cf. 34). Adams and Messick (4) proved that the *only* distribution function allowing the Luce model (51) and Suppes's monotone model (75), of which Thurstone's Case V is a special form, to fit the data simultaneously is the logistic. In view of the coordinating power of Luce's axiom and the negligible difference in practice between the logistic curve and the normal ogive, the use of the logistic for scaling is compellingly suggested. However, empirical comparisons (8, 43) of several different paired comparison models have resulted in remarkably similar scale values.

Categorical Judgment Methods

Scaling solutions for successive intervals have taken several forms under various names, but they reflect essentially the same basic model. Since some of these solutions (cf. 64) assumed equal stimulus dispersions, the flexibility of unequal dispersions (13) in the general model has been largely overlooked (74). The basic Thurstone formulation was axiomatized by Adams and Messick (4) and generalized to nonnormal stimulus distributions. Diederich, Messick, and Tucker (23) derived a weighted least squares solution for successive intervals which is applicable to incomplete data; the corresponding punched-card procedures were also outlined (57). Rozeboom and Jones (64) analyzed the effects of various errors upon successive interval scale values and found the scales to be stable under sampling fluctuations and insensitive to small departures from normality and equality of dispersions. Guilford (35) suggested a chi-square test of goodness of fit for successive intervals which inappropriately assumes independence of proportions across categories. Bock (9) proposed assign-

ing weights to ordered categories such that *n* judges classifying *m* stimuli *r* times each produce a maximum variance ratio for the stimuli. The technic permits the statistical rejection of deviant judges. Rosner (63) assayed a category judgment model in which scale values are directly calculable from the probability densities of stimuli over response categories. The derivation implicitly contains the restrictive assumption that all finite category widths are equal.

Morris and Jones (59) applied to preferences among "ways of life" in five cultures a factor analysis of successive intervals data. Kelley and others (44) noted that extreme discrepancies between equal-appearing interval attitude scales for Negro versus white judges were substantially decreased by successive intervals technics and almost eliminated by paired comparisons. Evidently judgmental distortion effects as a function of attitude do exist but are extremely subtle. Gardner and Thompson (30) carefully constructed interpersonal rating scales by having subjects select anchoring individuals from an outside reference group. Cliff (14), scaling adjectives and adjective-adverb combinations, obtained exceptional support for the hypothesis that the common adverbs of degree serve to multiply the intensity of adjectives.

Psychophysical Scaling

Stevens and Galanter (74) produced a pivotal monograph which differentiated psychophysical methods into two classes: those yielding "category scales" (equal-appearing intervals, successive intervals, paired comparisons, triads, and equisection) and those yielding "magnitude scales" (fractionation, doubling, constant sum, and magnitude estimation). The assembled data depicted category and magnitude scales as nonlinearly related to each other. From this the implication was drawn that category scales are artifactual, while magnitude scales are true scales. To forestall the reader from drawing the opposite conclusion, two factors tending to distort category scales were enumerated: heterogeneity of subjective category widths and of stimulus dispersions. One would look to the method of successive intervals to correct these two sources of distortion in equal-appearing intervals. Extraordinarily, the huge Stevens and Galanter compilation of category scales does not include any successive intervals analyses. It would be of utmost interest to apply directly to the assembled psychophysical data the method of successive intervals allowing unequal stimulus dispersions.

Controversy over the properties arising from magnitude scale methods still rages. Garner (31) pointed out that fractionation technics suffer from the unlikely assumption that the verbalized stimulus ratio corresponds to the *true* stimulus ratio and proposed a method using fractionation and equisection jointly. Apart from the content of the argument, the present reviewers lament the usage: *true* ratio scale. No single criterion for such absolute truth presently exists. Stevens (73) summarized evidence that

many psychophysical scales derived by magnitude methods approximate a power function law. Thus the magnitude methods are properly justified by their excellent ability to coordinate empirical data. Simple faith in ratio-scales, however, via the application of magnitude methods to expressive faces (28), neckties (24), and the like is not likely to prove especially rewarding.

Multidimensional Scaling

Many contributions to multidimensional scaling theory and method have followed the impetus given the field by Torgerson. Developments up to 1955 were reviewed by Messick (56). The multidimensional method of successive intervals (MDSI) was empirically evaluated (54) in the area of color perception by comparisons with scale values obtained from the Munsell color system and from the complete method of triads. The correspondence between these sets of scale values was exceptionally good, the proportions of common variance exceeding .95 (68). Mellinger (53) also applied MDSI to a variety of colors and obtained a separate dimension for each hue. However, the colors were extremely disparate, and the result would probably not have occurred with a more circumscribed set of stimuli. Agreement between *scaling in the large* with supraliminal differences and *scaling in the small* with confusions is not necessarily to be expected. Abelson (1) and Messick (55) used MDSI to compare attitude perceptions for two diverse groups. They both found merely minor variations between groups, suggesting that judges' attitudes distort only slightly their perceptions of attitude relationships.

Shepard (68) proposed a model for using confusion errors in paired associate learning as a basis for multidimensional scaling. He applied this important new technic to confusions among color stimuli, and the proportions of variance common with earlier analyses (56) exceeded .97. Ekman (27) scaled pairs of color stimuli with respect to similarity by the method of equal-appearing intervals and factored the obtained matrix of similarity scores directly! Such an analysis probably routinely results in spurious added dimensions. This criticism also applies to Andrews and Ray (6), who interpreted a simple square root function of $P(a,b)$ as a correlation coefficient to be factored directly. Wilson (78) defined a simple but somewhat arbitrary distance function relating squared distance to the proportion of times two stimuli are judged similar in the method of triads. Tucker (76) discussed a vector model for paired comparisons which yields a dimensional representation of the stimulus space as well as a group preference scale.

Guttman Scale Analysis

Guttman's coefficient of reproducibility (Rep) typically has a variously high expected value under the null hypothesis of item independence. Ob-

tained Reps are therefore apt to be misleading, and a number of alternative coefficients have been suggested. Green (33) proposed an "Index of Consistency" with the formula, $I = \frac{\text{Obtained Rep—Chance Rep}}{\text{Chance Rep}}$. Borgatta (10) recommended a different index based on much the same grounds. White and Saltz (77) reviewed many but not all the alternative coefficients and favored Green's because of an accompanying approximate significance test. Milholland (58) considered nonstandard assessments of reproducibility. His most intriguing suggestion was a coefficient giving the percentage of individuals whose response patterns fit perfect scale types, with multidimensional Coombsian scale types included. Slater (71) revived a technic, originally suggested by Guttman in 1941, that simultaneously assigns scores to people and to item responses according to a maximum variance-ratio principle. The technic applies to items with any number of alternatives. A significance test for scalability was given. Lord (48) showed that Guttman's principal components are the scoring weights that maximize the generalized Kuder-Richardson reliability coefficient and that the principal component for any dichotomous item is the same as the ordinary factor loading of the item divided by the item standard deviation. Applications of Guttman scaling were legion; space precludes their inclusion here.

Latent Structure Analysis

Anderson (5), Gibson (32), and McHugh (52) offered improved matrix solutions for latent class parameters. McHugh gave a significance test for the number of latent classes. Birnbaum (7) showed, for factorially homogeneous items whose trace lines are logistic curves, that a certain weighted average of a respondent's item scores is a sufficient statistic for estimating his score on the underlying common factor. Lazarsfeld (46) discussed various aspects of latent structure models. Hays and Borgatta (41) found that the general three-parameter solution in the latent distance model is to be preferred in practice to the restricted two-parameter solution.

The present reviewers were disappointed by the absence of substantive applications of the Lazarsfeld models in the available literature.

Criteria for Scale Evaluation

Traditional psychometric theory recognizes multiple criteria for evaluating a measurement procedure. Scaling theory should do likewise to avoid futile bickering as to whose numbers are on the side of the angels. Replicability of scales in repeated experiments is a universally recognized criterion of scale goodness. Beyond this, three other criteria by which to evaluate a psychological scaling technic should be mentioned: (a) Internal consistency. Does part of the scaling data appropriately predict remaining scale relations? Indeed, does the model provide any means for making such

predictions? (b) Coordinating power. Does the scaling technic yield a body of empirical scales which can be organized coherently? (c) Appropriateness as a psychological model. Is the subject's behavior treated consistently with what is known about similar human behavior in other psychological contexts?

In the opinion of the reviewers, the most crucial differential criterion of the three is the last, for it is on the basis of their implications for general psychological theory that different scaling models will ultimately prove their relative merits. We propose a tentative classification of existing scaling methods according to their psychological model of what the individual judge or respondent is "really" doing.

1. Deterministic models: Guttman, Coombs, Stevens, Siegel (69), Davidson-Suppes (20). The individual has a fixed "real" scale of judgment or preference, and his verbal reports reflect "real" metric properties.

2. Stochastic models

a. Roulette wheel or urn models: Lazarsfeld, Bradley-Terry (11), and possibly Luce (51). The individual acts as tho he consults a table of random numbers before making certain choices. The scale unit is a probability measure.

b. Confusion models: Thurstone and developments in his tradition. The individual acts as tho his momentary impression of the stimulus is subject to random fluctuations. The scale unit is a discriminal dispersion.

c. Dynamic vacillation models: No existing models altho Coombs (18) implied such a formulation. The individual acts as tho he *himself* shifts his standards of judgment or preference from moment to moment. The scale unit would presumably be a dispersion unit.

d. Noncommittal models: Suppes (75), Davidson-Marschak (19), and possibly Luce (51). The axiomatic systems postulate stochastic behavior without seeking an underlying psychological "explanation."

Deterministic and stochastic models each have their difficulties. The deterministic models are forced to make arbitrary assumptions about error, while the stochastic models often pool data from different individuals even tho for such areas as attitudes, personality, and preferences, the assumption of subject homogeneity is dubious.

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CHAPTER VIII

Research Tools: Statistical Methods

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THAT the rate of growth of statistical methodology has been nothing short of amazing during the past three years is immediately apparent to anyone who examines the amount of research published in the journals. Even within the somewhat restricted area of coverage represented by this chapter more than 750 references were located of which slightly more than 200 were included. In view of space limitations in the REVIEW it appears that chapters on research and statistical methodology must be increasingly selective. Altho rigid criteria were not set in the choice of books and articles to be reviewed, an attempt was made to cite those publications, relative to the scope of this chapter, that represent (a) a significant modification of aspects of statistical theory and/or (b) an important contribution of particular relevance to the analysis of data associated with research problems in education.

The last comprehensive single chapter on developments in statistical theory was the excellent one by Johnson and Moonan (105) in the December 1951 REVIEW. The December 1954 issue covered most of the publications in statistical methodology from 1951 to 1954. The portions of this chapter dealing with nonparametric methods, regression and correlation technics, and factor analysis represent extensions of the corresponding chapters by Blum and Fattu (22), Hoyt and Johnson (102), and Solomon and Rosner (179) in the December 1954 issue, and cover the three-year period following July 1, 1954. In addition, attention is given to a few earlier noteworthy contributions not previously noted in the REVIEW.

It should also be mentioned that a number of contributions to statistical methodology have been omitted in addition to those represented by the content of other chapters in this issue. In particular those statistical methods that seemed to be especially applicable to problems of test construction, analysis, and evaluation were deferred for a chapter in a subsequent issue on educational and psychological testing. Thus, many contributions in the area of regression and correlation as well as recent writings on pattern and profile analysis have not been included. Largely because of the somewhat limited amount of material available during the past three years, papers concerned with decision theory, the discriminant functions, sequential analysis, classification, and sociometric problems were not considered except incidentally. It would seem probable that within the next three years a sufficient accumulation of material on decision theory and discriminant analysis will be forthcoming to allow inclusion of a chapter on these topics for the 1960 issue of the REVIEW.

This chapter is organized as follows: After a review of recent books in statistics that are of interest and help to research workers in education,

major consideration will be given to (a) general developments in statistical theory with particular emphasis upon contributions to statistical inference involving parametric procedures; (b) recent contributions concerning chi-square and contingency tables as well as related topics; (c) nonparametric methods including some material on measures of correlation and association; (d) regression and correlation technics primarily viewed in a parametric setting; and (e) factor analysis in which, in view of space limitations, only methodological advances are treated.

Books

Altho during the period reviewed, many of the significant books pertaining to statistical methodology were devoted to experimental design, several other books in statistics were published that are of interest to research workers in the behavioral sciences. In statistical inference a potential classic is the volume by Fisher (62). Among introductory books of general scope are those by Adams (1), Dixon and Massey (52), Hoel (98), Li (122), Snedecor (178), and Wallis and Roberts (204). Of these texts the two by Dixon and Massey and Wallis and Roberts probably placed the least premium on background in mathematics altho two or possibly three years of college mathematics should suffice for comprehension of the material in any of the other volumes cited. Of all the books mentioned, perhaps the most useful one to the individual doing applied research is the revised edition by Dixon and Massey which, to say nothing of the unusual clarity of exposition of a variety of standard topics, contains 33 different tables. Other useful tables were compiled by Pearson and Hartley (148) from those appearing in various issues of *Biometrika*.

Probably the most elementary and perhaps one of the most readable statistics texts is that by Underwood and others (203). In addition, new volumes appeared by Cornell (43), Edwards (59), Tate (187), and Wert, Neidt, and Ahmann (209); they should be useful in two- or three-semester courses for upper division or graduate students in the behavioral sciences. Guilford (73) and McNemar (128) revised their well-known texts. As teaching aids to accompany the Guilford volume, Guilford and Michael (75, 76) prepared two workbooks that contain problems requiring step-by-step solutions as well as answers.

Important contributions of a more specialized nature were to be found in the areas of nonparametric statistics and factor analysis. In the former field, Fraser (65) wrote a mathematically rigorous text in nonparametric statistics which only a handful of specialists in the behavioral sciences will be able to comprehend. On the other hand, Siegel (177) prepared a comprehensive, lucid text; especially oriented to the needs of research workers in the behavioral sciences, it is probably the best current source of material on nonparametric methods for readers of the REVIEW.

In factor analysis Adcock (2) provided a brief and elementary book for the student with minimal training in mathematics. Relatively elementary and well suited to a three- or four-semester-hour course in factor analysis is Fruchter's text (66) that is essentially Thurstonian in its emphasis. An excellent chapter on factor analysis is to be found in Guilford's revised edition of *Psychometric Methods* (74), a volume which also treats several other quantitative methods from a somewhat applied point of view.

Not to be overlooked is the first volume of the *Handbook of Social Psychology*, edited by Lindzey (123), which contains several chapters on different aspects of statistical methods. Extremely useful sources of material on statistical theory and research design are the chapters that were prepared by Jones (108), Moses (138), and Gardner (67), respectively, in the 1955, 1956, and 1957 volumes of the *Annual Review of Psychology*.

Perhaps the one most intriguing book in statistical methodology during the past few years is Meehl's short but important *Clinical vs. Statistical Prediction* (131). In pitting actuarial and clinical methods against each other, the author took 20 studies in which both approaches were used to predict future behavior and compared the relative merits of each approach.

General Developments Primarily in Parametric Statistics

Outside the areas of regression and correlation and analysis of variance and covariance, published research tended to be nonparametric rather than parametric in its emphasis. Nevertheless a few noteworthy contributions to parametric theory appeared. Two highly readable papers of general interest were those by Chernoff (32) and by Tukey (202).

Chernoff presented a selective review of large-sample parametric theory. He also considered aspects of inference relative to the emphasis of the maximum likelihood principle in estimation and the use of optimal designs for estimating parameters and for testing both simple and composite hypotheses against appropriate alternatives. In his stimulating paper Tukey pointed to a number of unsolved problems of experimental statistics.

Contributions to statistical inference were made by Halperin (88) who presented charts for estimating parameters when sampling has occurred for a singly truncated normal distribution, and by Raj (153) who treated the estimation of parameters of a Type III population from singly and doubly truncated samples. Thru the use of a double sampling procedure for the estimation of population means relative to a given confidence level, Seelbinder (174) described a method for ascertaining the size of the first sample stage upon which the second sample stage is dependent; he furnished tables for determination of the optimal size for first-stage samples in terms of four different confidence coefficients. Owen (144) presented a double sampling procedure for testing the mean of a normal distribution; his method does not require so many observations as conventional single

sample tests for maintenance of the same degree of power. Readily extended to the case of the difference between two means, the test was treated from the standpoint of both known and unknown standard deviations, and its use was facilitated thru accompanying tables. In certain respects these double-sampling procedures constitute a simple decision-making process not unlike a rudimentary sequential analysis. Altho the topic of sequential analysis is outside the scope of this chapter, the interested reader is referred to a comprehensive article written by Fiske and Jones (63) especially for an audience of psychological research workers.

In a comprehensive paper Proschan (151) discussed at length the similarities and differences between confidence and tolerance intervals for the normal distribution in various cases of known and unknown means and standard deviations. Noether (142) treated two confidence intervals for two different expressions of corresponding ratios of probability estimates that may be interpreted as measures of effectiveness.

In two related articles Chernoff and Lieberman (34, 35) considered the use of probability paper. In their first article, the writers presented tables for samples up to size 10 as an aid to the selection of ordinates on normal probability paper that will permit "optimum" graphical determination of the mean and standard deviation of a normal distribution. Extending their problem to the case of a general continuous distribution with finite variance that is completely specified except for location and scale parameters, the writers in their second paper stated in abstract terms necessary and sufficient conditions to guarantee optimal estimates not only of the scale parameters, but also of each of the percentiles.

Bridging to some extent the gap between parametric and nonparametric approaches are order statistics (e.g., percentiles or linear combinations thereof) used to estimate the parameters of populations of specified form. The lucid exposition of order statistics and the extensive tables furnished by Dixon and Massey (52) are adequate for the purposes of many educational research workers; in addition, important theoretical contributions to the estimation of parameters from combinations (usually linear) of order statistics appeared. In a series of closely related papers Sarhan (163, 164, 165, 166) employed order statistics in estimating parameters of various types of distributions, and subsequently Sarhan and Greenberg (167) wrote a theoretical paper concerning the use of order statistics in the estimation of parameters in singly and doubly censored samples. Relative to the problem of missing or censored observations, Sarhan and Greenberg (168) published tables for estimation of parameters for samples up to 10 in size. For samples of size 20 and less from a normal distribution Teichroew (193) reported the expected values of order statistics and of the products of order statistics, and furnished tabulations.

Additional miscellaneous contributions to statistical theory were Godwin's expository and detailed paper (69) on generalizations of inequali-

ties of the type set forth by Tchebychef as applied to a variety of distribution functions, Tukey's suggestions (201) for maintaining simplicity in moment-like sampling computations, Sandler's arithmetical simplification (162) of the *t*-test of the significance of the difference between correlated means, and Teichroew's listing (192) of a number of unpublished statistical tables prepared by members of the Numerical Analysis Institute of the University of California at Los Angeles. Of possible interest to people in the mental hygiene field was the development by Marshall and Goldhamer (129) of three statistical models based upon application of Markov processes that may be used in the study of such variables as the age of onset of a psychosis and the age of admission to a mental hospital.

Finally mention should be made of a running controversy concerning the use of either one-tailed or two-tailed significance tests that appeared in at least seven different articles in the *Psychological Review* and *Psychological Bulletin* between 1951 and 1954. In two reviews Jones (108) and Moses (138) cited pertinent references and summarized and evaluated the arguments presented in the original sources.

Chi-Square, Contingency Tables, and Related Topics

Extending his treatment in an earlier comprehensive article (36) dealing with chi-square, Cochran (37) wrote a highly substantive paper concerning ways in which applications of common chi-square tests may be strengthened. He discussed such problems as goodness of fit, subdivision of degrees of freedom in the detection of a linear or other type of trend in a contingency table, and over-all significance tests for combinations of 2 x 2 contingency tables. After making the point that chi-square as a test of goodness of fit gives us no indication of how a null hypothesis fails because it is not directed against any particular pattern of deviations of the observed from the expected frequencies, Cochran (38) suggested a new procedure called the *L*-test, which is a linear function of the deviations between a set of observed frequencies and another set of corresponding frequencies so chosen in advance that *L* will be sensitive to the alternative hypothesis thought most likely to hold. Altho approximate except in the asymptotic case, the *L*-test can be made responsive to any specified pattern of deviations with respect to either their signs or magnitudes.

An important paper also concerned with goodness of fit was that by Chernoff and Lehmann (33) who demonstrated that chi-square for grouped frequencies follows the calculated chi-square distribution provided the estimates of the parameters employed in the calculation of expected frequencies constitute maximum likelihood estimates. When the sample mean and standard deviation are used in the test for normality of distribution, there is a tendency for the value of chi-square to be overestimated especially in the instance of a small number of cells. In a recent short article concerning the interpretation of chi-square tests with respect to a study of the preference of 31 subjects for two drugs administered on two

occasions, Armitage and Healy (8) compared goodness-of-fit tests and variance tests with exact tests and showed in their tabulation that the probability values closest to the .05 point turned out to be .018, .064, and .0415, respectively, for the three approaches.

Berkson (16), dealing with a rather specialized problem in bio-assay work involving the logistic function, reported the results of a series of experiments that cast doubt upon the widely held view of the efficiency of maximum likelihood estimates when compared with the minimum Pearson chi-square estimates. In particular his findings conflicted with the more or less commonly accepted principles that (a) a sufficient estimator is either unique or functionally related to a maximum likelihood estimator and (b) the maximum likelihood estimator in the instance of asymptotically efficient estimators will extract the greatest amount of information from the data.

Significance Tests

Relative to the performance of significance tests of individual 2 x 2 contingency tables, several helpful papers were published. Applicable not only to fourfold tables, but also to those problems the data for which are compared with the chi-square model, are the important tables prepared by Lewis (121) that furnish the 0.1 and 99.9 percent points of chi-square over an extensive range of degree of freedom. Tables prepared by Finney (61) are useful in yielding an exact test of association for 2 x 2 tables at .05, .025, .01, and .005 levels of significance in the instance in which both frequencies present in one of the margins are less than or equal to 15; Latscha (120) extended these tables so that both marginal frequencies go up to 20. In the instance of a 2 x 2 contingency table that includes up to a total of 50 observations Armsen (9) provided, for both one-tailed and two-tailed tests at the .05 and .01 levels, a set of exact probability tables derived from use of a hypergeometric formula.

Making use of binomial coefficients instead of factorials in order to shorten the calculation of exact probabilities for either 2 x 2 or 2 x r contingency tables, Sakoda and Cohen (160) furnished a table of binomial coefficients accurate to four significant figures for n between unity and 60 as well as a set of inequalities for estimation of cumulative probabilities in the tail of the binomial distribution relative to a given set of entries in a contingency table. Another group of tables very useful when the proportions p_1 and p_2 of individuals in each of two samples belonging to a given category are very small (altho the numbers of individuals N_1 and N_2 in the two samples are relatively large) was prepared by Patnaik (146) on the hypothesis that the two samples are drawn from a common Poisson population.

Quick graphical methods for the evaluation of the significance of entries in 2 x 2 contingency tables were described in two papers by Bross and Kasten (26) and by Trites (197). In the first mentioned paper, charts are furnished that permit both one-tailed and two-tailed significance tests

without the need for calculation of chi-square provided the proportion of cases in either of two samples being compared is between .10 and .90 of the combined number of cases in the two samples. Altho minimizing computational effort, the sets of curves in the second paper are strictly applicable only when the numbers in each of the two samples are the same.

As to significance tests involving proportions and percentages Gengerelli and Michael (68) proposed a procedure for evaluating the reliability of the difference between proportions and for setting up confidence intervals. In a paper also concerned with the estimation of binomial parameters Bross (24) devised a method based on the construction of a confidence interval for ascertaining whether a sample proportion p_2 is significantly larger by a certain percentage than another sample proportion p_1 as given by $\Theta = 100\ (p_2 - p_1)/p_1$ upon the assumption that both p_1 and p_2 are sufficiently small and that the samples are sufficiently large to permit use of a Poisson approximation.

Crow (46) described how confidence intervals could be established for a proportion. Rao and Chakravarti (155) developed significance tests for the Poisson distribution in the case of small samples.

Combining Independent Tests of Significance

For the important problem of evaluating a series or combination of independent significance tests, Jones and Fiske (109) proposed and described in detail a binomial model and a chi-square model involving a logarithmic transformation of the product of the independent probabilities p associated with each of the k number of independent significance tests. That each of the sets of measures must be independent was emphasized in application of the models altho substitute procedures were suggested when statistical independence could not be assumed. Well adapted to the calculation of numerical values of chi-square arising from use of the second model was a set of 2 x k tables prepared by Gordon, Loveland, and Cureton (71). Perhaps of even more help was the appearance of two nomographs prepared by Sakoda, Cohen, and Beall (161) that furnish chance probabilities of obtaining n or more statistics significant at the .05 or .01 levels, respectively, from N calculated statistics.

For the situation in which an investigator wishes to test the over-all significance of a set of 2 x 2 contingency tables, Yates (215, 216) cited reasons why he believed the previously mentioned combination of probabilities test (which is compared with the chi-square distribution consisting of $2\ k$ degrees of freedom) is not too efficient relative to a maximum likelihood solution involving the use of some appropriate transformation, but nevertheless concluded in his second paper that "chi-square without correction for continuity will give one-tail probabilities for 2 x 2 tables which may be safely combined in most cases" encountered in practice. In his previously cited paper Cochran (37) considered the circumstance in which the investigator is interested in ascertaining an over-all trend of

differences in a series of independent pairs of proportions arising from some natural ordering of events and proposed a significance test for the corresponding combination of 2 x 2 contingency tables, a test that anticipated an equivalent procedure suggested by Armitage (7).

Writing a comprehensive theoretical paper concerning the combination of independent tests of significance, Birnbaum (17) evaluated several different approaches that had been proposed and concluded that since no single method of combination is in general optimal, attention should be given to the kinds of tests to be combined in the selection of a method. However, for one kind of common problem it was shown that methods by Fisher and by Tippett possessed an optimal property. Finally for the reader desirous of an overview of the problem of combining independent tests of significance, the detailed three-page discussion by Mosteller and Bush (140) may well be the most helpful single source.

Miscellaneous Developments

Two noteworthy contributions regarding the separation of the total chi-square in contingency tables into components may be briefly cited. Kimball (118) proposed short-cut computational formulas that allow the partition of the chi-square value in a multifold contingency table of r rows and s columns into the chi-squares associated with various $(r - 1)$ $(s - 1)$ individual 2 x 2 tables. After drawing an analogy between factorial experiments involving use of analysis of variance and multiple contigency tables in which the nature of measurements prevents their being scaled, Sutcliffe (185) furnished a model for the partition of chi-square in multiple contingency analysis into components associated with each of the main effects and with various orders of interaction, a model which is detailed relative to different types of restriction dependent upon whether parameters are known or estimated.

Bross (25) indicated that when errors of classification arise in 2 x 2 tables as in clinical diagnosis, such errors may be relatively more serious in their influence on the estimation of parameters than is their attenuation of the accuracy of significance tests (as in the instance of the difference between two sample proportions) altho the power of the significance test will suffer. For the situation in which frequencies are missing in contingency tables, Watson (206) proposed a "missing value" formula based on the principle of maximum likelihood to compute estimates for all unavailable frequencies and also considered the problem of "mixed-up" frequencies.

What may be considered a critical overview of the use of contingency tables will be found in a paper by Goodman and Kruskal (70) and in one by Mayo (130). The first two writers in a lengthy discussion proposed several new indexes for contingency tables (i.e., those of multiple and partial association in more complex arrangements) and distinguished between ordered and unordered variables and symmetric and unsymmetric problems upon the assumption of no underlying continuum being

present. Mayo (130) considered some recently developed technics for the analysis of association in contingency tables, illustrating his procedures with data from a follow-up survey of education graduates. He discussed approximate tests of significance for detection of departures of a specified type such as the presence of correlation or regression in certain parts of a contingency table, tests of higher order interactions, and exact tests of significance in the instance of small sample data or in the case of the presence of small or even zero theoretical frequencies in contingency tables.

In order to meet the condition in which nonindependence of the marginal distribution of a two-way m x m classification may exist, Stuart (184) devised a large sample test for the homogeneity of two marginal distributions. In still another paper in which the presence of correlation is an important factor Stuart (181) furnished a means for comparing the frequencies (with respect to possession of a given attribute) in matched samples.

Several papers concerning Poisson processes and the binomial distribution appeared in addition to those previously cited. In a comprehensive article Birnbaum (18) described and illustrated statistical methods that can be applied to Poisson and experimental processes. For samples that are truncated and censored, Cohen (39) proposed maximum likelihood estimates of Poisson parameters and explained how existing tables could be used to apply the formulas to practical problems. As a means of stabilizing variances and normalizing distributions Blom (21) compared various transformation procedures for the binomial, negative binomial, Poisson, and chi-square distributions. To describe the theoretical extent of accident proneness in terms of two independent periods of accident observation, Webb and Jones (208) proposed what they believed to be two operationally comparable models, the Poisson distribution and the binomial bivariate (correlation) method, the mathematical equivalence of which was proved by Burke (27). In addition, striking relationships to the Spearman-Brown reliability estimate were demonstrated.

Developments in Nonparametric Statistics

In addition to the previously cited writings on nonparametric statistics by Siegel (177) and by Mosteller and Bush (140), mention should be made of a very simple, lucidly expressed, nontechnical article by Siegel (176). Several other general contributions to the logic and theory of nonparametric statistics that either represent important advances from a mathematical standpoint or suggest problems in need of solution should be cited.

Using set theory, Bahadur and Savage (11) demonstrated the absence of certain statistical procedures in nonparametric problems. It was argued that if the distribution of a real random variable in a population is totally unknown, little or no information about the tails of the population can

be furnished by a sample even if it has been drawn according to a sequential method. In their treatment of problems of inference about the population mean the writers pointed out that no effective test of the hypothesis of the population mean being equal to zero can be made, that no confidence interval can be established, and that no point estimate is possible since the parameter is sensitive to the tails of the population distribution. Concerned with the glibness or apparent lack of interest that statisticians show in treating ties in nonparametric procedures, Putter (152) showed that when ties are considered as being random variables as compared with their being treated as nonrandomized variables, the exact power and asymptotic efficiency of the tests are reduced. In essence he proposed a nonrandom model for treatment of ties.

In a valuable theoretical paper on rank order statistics Savage (170) pointed out that in nonparametric work it is seldom possible to apply all the basic ideas and related concepts pertaining to the testing of alternative hypotheses underlying the parametric procedures of Neyman and Pearson since for many of the alternatives considered in nonparametric methods neither optimum critical regions nor analytic technics for determining power functions may exist. For the two-sample case Savage considered alternatives involving monotone likelihood ratios and presented a necessary criterion for admissibility. Another theoretical contribution was Dwass's paper (57) concerning the distribution of ranks and of chosen rank order statistics. Subsequent to developing the moment generating functions associated with two sets of independent sets of ranks from two possibly different populations, he showed the Wilcoxon statistic to be a special case of the general distribution statistic of rank orders and demonstrated that for certain combinations of sample sizes and parent populations the limiting distribution is nonnormal.

Tests of Significance

As might be expected, several new nonparametric significance tests or adaptations of existing tests appeared. In the comparison of two samples for significance Moses (139) proposed a test, based on the ranks of observed values, that is sensitive to the presence of extreme scores (either large or small or both large and small). The test was devised to take care of the situation involving experimental and control groups in which the effect of the experimental variable may be reflected by an increase in the scores of some subjects and by a decrease in the scores of other subjects. Employing a criterion based on the rank ranges of two samples, Kamat (117) presented a new distribution-free test for samples with respect to which it is assumed that no difference exists in the location parameters of the two population distributions.

For the circumstance in which two variables are measured on each individual, Hodges (96) developed a bivariate analog of the two-sided sign test. In testing the null hypothesis that the bivariate distribution for one pair of variables (x_1, y_1) is identical with that of a second pair of

variables (x_1', y_1') on the assumption that individual vectors are independent, attention is directed toward whether in a second circumstance the bivariate distribution has shifted relative to its position in the first circumstance in a direction generally the same for all individuals. In presenting a new distribution-free test along with appropriate tables of significance and power for the hypothesis that given values exist for all regression parameters, Daniels (49) assumed that the probability of the signs of the independent residuals being positive (or negative) is one-half. Altho his test statistic was determined explicitly for two parameters, it can be extended in principle to any number of variables.

Several modifications in, or additions to, existing significance tables appeared. To assist the research worker who wishes to make use of the *G*-test (an approximation to the *t*-test, the denominator of which is replaced by the average range of the combined number of samples *m*), Jackson and Ross (103) presented a set of tables that yield 10, 05, and 01 percentage points in two-tail tests for situations permitting various numbers of *m* random subgroups each of size *n*. For a nonparametric test of location Rosenbaum (159) prepared an extensive set of tables that merely involve a count of the number of points in one sample that fall outside an extreme value of the second sample. For determination of significance probabilities associated with the Wilcoxon test, Fix and Hodges (64) furnished tables that cover a range between 2 and 12 for the smaller sample size. Percentage points for the Kolmogorov statistics are available in a table prepared by Miller (134). In the instance of a distribution-free test of goodness of fit, Anderson and Darling (6) tabulated large sample significance points.

Power and Efficiency

The problem of the power and efficiency of nonparametric tests was the stimulus for several studies. In a comprehensive paper on the asymptotic efficiencies of five two-sample nonparametric tests against normal alternatives to the null hypothesis, Mood (135) determined asymptotic efficiencies of $3/\pi$ (about 95 percent), $2/\pi$ (about 64 percent), and zero, respectively, for the Wilcoxon test for location, the median test for location, and two run tests. For a square rank test of dispersion an efficiency of $15/2\pi^2$ (about 76 percent) was reported.

For samples of various sizes Dixon (50) tabulated the power functions of the sign test when α is near .05 and .01 and employed a power efficiency function, which is the ratio of the size of sample drawn from a normal population when the *t*-test is used to the size of sample in the nonparametric test if equal power is maintained relative to a given alternative. The tabulated results demonstrated that the power efficiency decreased as the size of sample increased, as the level of significance increased, and as the extent of departure δ of the alternative hypothesis from the null hypothesis increased. A second paper by Dixon (51) concerning the power efficiency of four nonparametric tests for two samples of size $m =$

$n = 3, 4, 5$ against normal alternatives revealed, in terms of exact results, that the most efficient test is the Wilcoxon (rank sum) test followed by the Kolmogorov-Smirnov (maximum absolute deviation) test, and the median test. As the value of the alternative hypothesis becomes more remote from that of the null hypothesis, the efficiency tends to drop only slightly, but relatively more for the first two tests than for the median test.

In studying the efficiency of nonparametric competitors of the *t*-test, Hodges and Lehmann (97) showed that the asymptotic efficiency of the two-sample Wilcoxon test relative to the *t*-test never falls below .864 and obtained the limit of a sequence of power efficiency functions for the sign test with respect to t in normal populations as the size of samples becomes infinite. In other theoretical papers Tsao (199) proposed methods to approximate the distribution of ranks that could be used as a basis for the evaluation of the power of an arbitrary rank test, and Dwass (56) studied the large-sample power of certain rank order tests in two-sample problems against alternatives involving one parameter.

In an empirical study of the power of the rank, median, and run tests in which ties are numerous Tate (186) found that the degree of power was highest for the rank test, slightly lower for the median test, and extremely low for the run test. A related contribution was that of Teichroew (191) who furnished empirically derived tables of power functions of any rank order test of the hypothesis that the two samples come from the same population when the sizes of the two samples are 2 and 3, 3 and 3, 2 and 4, or 3 and 4.

Tests of Randomness

Concerning nonparametric tests for the hypothesis of randomness in a sequence of values several important contributions were made. Perhaps of greatest theoretical interest was Savage's demonstration (171) of the independence between rank order statistics and symmetric statistics such as the *t*-test. He concluded that if random variables are independently and identically distributed as in random samples, the statistics employed to test randomness and parametric features of the null hypothesis continue to be independent even tho the parametric parts of the null hypothesis may be false. For example, one might test a set of observations for both randomness and normality. The nonparametric test of randomness would probably depend upon an alternative of interest relative to which a form of rank order correlation would be appropriate. However, the test of normality in which the classical chi-square test of goodness of fit should be employed involving the estimation of one parameter would be completely independent of the nonparametric test of randomness.

Among related papers of interest upon randomness was one by Stuart (183) who presented tables of the asymptotic relative efficiencies of various tests of randomness against normal regression alternatives. Thru making use of Kendall's tau coefficient, Jonckheere (106) not only ob-

tained a general statistic S that could be used to test the extent of agreement between hypothesized rank-order values for n objects or scores and an entire set of observed rankings of the same n objects or scores by m judges, but also furnished a useful group of tables. As an alternative statistic Lubin (127) proposed one he designated as J as a rank order test for trend in correlated means.

To test the hypothesis of randomness of sequence of N observations, or equivalently the hypothesis that N independent random variables possess the same continuous distribution function, Cox and Stuart (45) presented several practical quick sign tests for trend in location and dispersion altho they pointed out that even their best tests, which may be preferable to other simple tests in the literature, are less efficient than the rank correlation tests. In a subsequent note Cox (44) suggested that when an inefficient quick test is compared with an efficient test such as t, consideration should be given to the extent to which agreement is found in the application of the two tests to the same set of data. In describing a model based on the distortion of a sequence of random values, Barton and David (13) suggested its use as an alternative to the hypothesis of randomness, applied it to Spearman's rho coefficient, and stressed its usefulness as an alternative to the random sequence associated with any bivariate criterion.

The Tau Coefficient

The nonparametric coefficient of association, Kendall's tau coefficient, was the object of extensive study. Haberman (87) described distributions for the coefficient that were based on partially ordered systems. Previously Stuart (180) succeeded in establishing bounds for the sampling variance of the coefficient. With no assumptions about the nature of the scales of measurement (other than for the existence of ordinal values), the shapes of distributions involved, or the comparative standing or variability of different subgroups, Torgerson (196) generalized Kendall's test of association between two sets of rank orders to the case in which the total sample consists of several subgroups of variable sizes and in which data on one or both of the variables to be correlated are made up of rank orders within each subgroup. Moreover, he demonstrated thru two empirical examples that a normal approximation to the exact test of significance will suffice in most practical situations. Both Torgerson (196) and Adler (3) proposed methods for handling ties. Challenging Schaeffer and Levitt (172) who in their comprehensive review of the literature on Kendall's tau coefficient stated that "generally applicable tests of the significance of any partial τ are not yet available," Jones (110) presented a significance test in the instance of a partial tau relative to which the partialed-out variable is nominal in its scale properties.

Noteworthy advances were made in the development of improved methods of computation of tau such as the simplification proposed by Bright (23) and Cartwright (29). Reducing the labor involved in Bright's

approach, Griffin (72) described a graphic method for calculation of the coefficient.

Several miscellaneous developments in nonparametric methods of interest to research workers in education and psychology were those of Jonckheere (107) who described a test of significance of the relationship between the predicted grading of n objects into k ordered categories and the corresponding rankings of the n objects by m judges, and of Cartwright (30) who developed a quick estimate of multijudge reliability. Cureton (48) developed a formula for rank-biserial correlation which was shown to be equivalent to both Kendall's tau and Spearman's rho coefficients. Stuart (182) demonstrated that for samples drawn from normal and from uniform distributions the correlations between variate values and ranks are .94 and .96, respectively, when N is equal to 25; the correlations have .98 and 1.00 as upper bound values as N increases.

Regression and Correlation

In correlation and regression theory a variety of problem areas was studied. In distribution theory Dunnet and Sobel (53) considered a multivariate generalization of Student's t distribution, and in treating the bivariate case in detail, obtained exact and asymptotic expressions for the probability integral as well as an asymptotic expression for certain percentage points. In a comprehensive paper on multivariate distribution theory Olkin and Roy (143) derived the sampling distributions of a broad group of statistics directly from the probability law for random samples drawn from a multivariate normal population; they showed the application of their new derivation to situations concerned with canonical correlations and multiple and partial correlations.

Another important contribution to multivariate analysis was Pillai's proposal (149) of three new test criteria that were based on the characteristic roots of matrices derived from the product-moment matrices corresponding to samples chosen from multivariate normal populations. The approximate distributions of the statistics concerned were seen to conform to those of Type I or Type II Beta. In line with his statement that the distribution laws of random variables are possibly relative to the assumption of an appropriate kind of stochastic dependence on linear statistics, Laha (119) was able to characterize the normal distribution in terms of the properties of both linearity of regression and homoscedasticity of suitable linear statistics. The distribution of the regression coefficient in samples arising from a nonnormal population was the subject of a study by Hill (95) who in addition investigated the influence of departures from normality upon the significance levels furnished by two t-tests involving the regression coefficient. It would appear in the instance of samples of size 11 or larger that the magnitudes of the discrepancies attributed to nonnormality are constant and determinable from calculation furnished by knowledge of six cumulants. Finally, Harley (92) demonstrated a func-

tional relationship between the distribution of noncentral t and an expression for a transformed correlation coefficient that arises from a population in which the correlation is zero.

Parameters

The estimation of parameters in correlation and regression theory was a problem central to a number of papers. Using the method of maximum likelihood, Cohen (40) estimated the parameters of the bivariate normal distribution from restricted samples that arise when certain individuals have been eliminated in the selection process, and also illustrated his procedures. In the instance of the trivariate normal population both Edgett (58) and Lord (124) independently employed maximum likelihood estimators for determination of the parameters when some of the sample observations for one of the variates are missing and found explicit solutions to the maximum likelihood equations. Another application of the principle of maximum likelihood for a multivariate normal distribution when some of the observations are absent was subject to abstract discussion by Anderson (5) for the simplest case concerning the bivariate normal distribution. For the situation in which all individuals in the population have fixed, identical values in each of the independent variables X_1, X_2,—, X_k, Johnson (104) derived both a best linear estimate of the true mean score on the dependent, or predicted, variable, which was in essence an average of all predicted values, and a standard error of estimate.

Confidence Intervals

Determination of confidence intervals in estimation problems involving regression was the subject of papers by Durand (54) and by Crow (47). After discussing the nature of two fallacies usually committed in the determination of conventional confidence intervals for the partial regression coefficients of the multiple regression equation, Durand proposed a possible remedy thru the use of joint confidence regions that would seem more suitable to regression problems involving the estimation of several parameters. For the ordinate of the regression line or surface, Crow formulated a generalization of the confidence interval that permits the independent variable to assume values at random.

Serial, Biserial, and Point Biserial Coefficients

Important theoretical considerations concerning the biserial and the point biserial coefficient were treated in three papers by Tate (188, 189, 190). Altho his findings are too numerous to be cited in detail, a few points may be mentioned. In his first paper Tate showed that the point biserial coefficient is t distributed when the parameter is equal to zero; in his second article he proved that the asymptotic variance was a minimum for a fixed value of the parameter in the instance of a 50-50 split in the dichotomized variable; and in his third presentation the writer in citing applications of correlation models for biserial and point biserial co-

efficients compared the circumstances under which one of the coefficients would be preferred to the other; he recommended that if any doubt regarding the feasibility of a biserial coefficient should exist, one should then make use of the point biserial coefficient.

In a group of closely related papers on serial correlation Hannan (89, 90) and Watson (207) investigated the correlation of errors in a series of ordered observations. In the second cited paper Hannan not only described an exact test for the serial correlation present in residuals from the regression, but also developed an estimator of the regression coefficient. Watson showed that if the errors in the covariance matrix are independently and normally distributed in a homogeneous manner, an hypothesis regarding regression coefficients could be evaluated thru use of a test statistic with t or F distributions. Recently, in still another paper, Hannan (91) described how serial correlation may be tested for in least squares regression.

Regression Analysis

In the area of regression analysis several contributions that might be viewed as somewhat more applied than theoretical were made. Guion (77) presented a workable solution to the prediction of a quantitative variable from a weighted combination of qualitative or categorized variables and illustrated his procedures altho significance tests were not cited. In the instance of a two-way classification in the dependent or criterion variable, Michael and Perry (133) demonstrated both algebraically and numerically the comparability of the simple discriminant function and multiple regression technics. Thru a large-scale empirical study involving selection devices in education, Sevier (175) tested the assumptions underlying multiple regression and concluded that while the existence of both linearity and homogeneity could usually be justified, there was considerable doubt as to the tenability of the assumption of normality.

Computational Aids

Many computational aids in the form of short-cut numerical procedures, nomographs, charts, and tables appeared that should be of considerable service to individuals in applied statistics. In the area of multiple regression analysis Moonan (136) provided an arithmetically simple procedure for giving a quick estimate of a multiple correlation coefficient and verified empirically the feasibility of its use. It would seem to work best when the intercorrelations among the predictor variables are relatively homogeneous. Another contribution to multiple regression analysis was Lord's nomograph (125) for calculating the multiple correlation when there are two predictor variables.

Employing the criterion of least squares, Askovitz (10) developed a short-cut graphical procedure for fitting a straight line to a series of points, provided they are equally spaced on the axis representing the independent variable. To fit an asymptotic regression curve, Patterson (147) proposed a simple method. For the computation of correlations based on Q-sorts

Cohen (41) derived a useful equation and published a corresponding nomograph.

From four theorems stated by Reiersøl (157) showing the relationship of sign properties of zero-order correlation coefficients to those of partial correlation coefficients, logical deductions could be made regarding what the signs of partial correlation coefficients should be. Michael and Caffrey (132) described tables to facilitate calculation of first-order partial correlation coefficients. Perhaps one of the most useful groups of correlation tables is the set that Owen (145) prepared for computation of bivariate normal probabilities.

A special correlation procedure was derived by Winer (210) to indicate the amount of interrelationship between overlapping groups of individuals several of whom may belong to two or more groups in an organization of complex structure. The index proposed, which is equivalent to a product-moment coefficient, is not only lacking in certain indeterminancies usually inherent in the creation of fourfold tables pertinent to organizational analysis problems, but also fairly insensitive to the absolute size of totals in the margins of fourfold tables.

Factor Analysis

During the period July 1, 1954, to July 1, 1957, there was no lessening of interest in factor analysis as a research technic. So overwhelming was the number of papers that only a few of the most important methodological contributions can be reviewed.

Communality Estimation

The communality problem, the resolution of which is essential to the proper application of factor analytic methods, has probably been attacked with more vigor than during any previous three-year period since Spearman launched factor analysis more than 50 years ago. A truly major addition to the not inconsiderable literature concerning communalities was that of Rao (154) who answered two statistical questions. First, how can an estimate be obtained of the minimum rank of the variance-covariance matrix of the common parts of the tests, that is, of the so-called reduced correlation matrix? Second, how can the hypothesis that the rank of the population matrix is less than or equal to this estimated rank be tested against the alternative that it is greater? Thru applying the theory of canonical correlation to obtain a vector basis for a common-factor space Rao was able to answer the questions posed and to show that his approach furnishes one of several possible solutions to Lawley's well-known likelihood equations. Altho a firm statistical basis was furnished for determining a common-factor space of a testable number of dimensions, the calculations involved in Rao's procedures for a correlation matrix of even moderate size appear to be almost beyond the practical capabilities of most available electronic computers.

Altho many years ago Guttman (83) demonstrated that under rather general conditions the communality of a variable is equal to its squared multiple correlation on the remaining variables in the battery as the number of these variables becomes arbitrarily large, more recently he (78) reviewed four approaches that have been used to approximate communalities and gave a series of theorems and most compelling arguments for using as a best possible approximation his well-known lower bound, the squared multiple correlation. Recently Guttman (85) offered simple proofs of relations between communalities and multiple correlation properties. In another paper Guttman (82) proposed improved bounds for communalities, and more recently he (79) modified a well-known criterion to achieve communalities that maximize determinancy.

In stipulating the composition of hypothetical variables beyond those in the immediate battery, Tryon (198) and Kaiser (115) attempted to calculate the squared multiple correlation in the limit. Their results appear to be somewhat encouraging. With no a priori specification of rank, they seemed able to obtain a solution for unknown unique minimum rank communalities. Additional work by Kaiser (112, 113) indicated that the Tryon-Kaiser approach breaks down when minimum rank communalities are not unique, a circumstance which will invariably arise in practice.

From an examination of the ordinal properties of the factor-analytic model Bennett (15) set forth a method for determining the dimensionality of a score matrix, and Warburton (205) proposed a return to Hotelling's original formulation concerning the use of unities as communality approximations. In one of his final papers Thurstone (195) suggested that the arbitrary factoring of a correlation matrix be carried out according to the criterion that the factors to be extracted minimize the sum of the squares of the off-diagonal residual covariances. Altho not computationally feasible, this method of factoring does not require that communalities be known or even approximated; they fall out as an ex post facto byproduct of the factoring procedure.

In relation to the frequently used statistical procedure of separating a set of data or scores into two or more parts, Harris (94) showed that in factoring, use of unities in the diagonal cells and employment of communalities leads to a separation of the data into two orthogonal parts. Subsequently Harris (93) was concerned with the development of a transformation procedure for deriving factors based on communality estimates from those obtained when unities are employed, a procedure which, when applied to observed data, yields common factors that are consistent with Lawley's requirements for a maximum likelihood solution.

In two truly heroic efforts Wrigley (211, 212), with the aid of an electronic computer, attempted to follow thru on textbook recommendations that an ideal factoring procedure would be to compute iteratively principal axes for a given number of factors until the communality approximations converge. His results were not encouraging. With a correlation matrix of order 11, hundreds of successive principal axes solutions were necessary.

Not only did different solutions result from different initial approximations, but also in the matrix selected for investigation the Heywood case (a value for a communality in excess of unity) occurred more often than not. In a nonmathematical article Wrigley (213) discussed in detail some distinctions between common and specific variance and concluded that the most nearly appropriate approximation to the communality of a test is indeed Guttman's lower bound altho he indicated that it would be desirable to examine more closely Rao's statistical procedures.

From a purely algebraic viewpoint Guttman (86) considered the general question of rank reduction of correlation matrices when communalities are used. His most interesting finding was that there are classes of matrices, the rank of which may be reduced by only one—a development revealing that the implications of some earlier work by others on rank reduction are, in fact, false.

After reviewing the major mathematical results on the communality problem thru 1956, Kaiser (112) concluded that the traditional minimum rank formulation of the problem is inadequate and that Rao's recent statistical work (154) provides the essential missing ingredient to a problem improperly considered algebraic. Kaiser also delineated the psychometric approach to the communality problem which he described as the drawing of inferences regarding a universe of psychological variables from a particular representative sample or battery of variables chosen from this universe.

Rotation

One of the most exciting developments in factor analysis during the period reviewed was the rise of analytic rotational criteria that yield unique and objective solutions as contrasted with the subjectivity of graphical procedures. The first of these analytic criteria was proposed by Carroll (28), whose paper was reviewed in the December 1954 issue of the REVIEW. Subsequently a large number of related papers appeared, including one by Thurstone (194) yielding a semianalytic solution that was also cited in the December 1954 issue of the REVIEW.

Neuhaus and Wrigley (141) developed what was referred to as the "quartimax" method in which it was suggested that the "distribution" of squared loadings should be maximum as a means of achieving simple structure. Simultaneously Saunders (169) presented his "maximum kurtosis" criterion for orthogonal factors in which the kurtosis of the "distribution" of factor loadings and their reflections should be a maximum. Based on an analogous situation in information theory, Ferguson's proposal (60) was that an ideally parsimonious factor matrix is one in which the sum of the fourth power of the loadings is maximized. Amazingly in the orthogonal case, the criteria of Carroll, Neuhaus and Wrigley, Saunders, and Ferguson are all mathematically equivalent.

The problem of achieving an analytic criterion for the oblique case proved to be considerably more laborious. Because the form of his criterion

explicitly concerns interfactor relationships, the version proposed by Carroll was perhaps the most easily generalized for nonorthogonal factors. Kaiser (114) developed a mathematical solution of Carroll's criterion for the oblique case. Almost simultaneously Pinzka and Saunders (150) effected a slight modification in Saunders' orthogonal criterion that made an oblique solution possible.

Noticing a systematic bias in the orthogonal quartimax solutions as a mathematical explication of simple structure, Kaiser (111) proposed his "varimax" method. He argued that the quartimax method, rather than simplifying the loading profile of a factor, concentrates its attention on simplifying the factorial composition of a test. Consequently, he revised the quartimax method by requiring that the variance of the squared loadings be maximized by factors rather than by tests. His numerical results support his hypothesis. Later Kaiser (116) improved the psychometric invariance properties of his criterion thru carrying out rotations with respect to normalized loadings. By requiring that the sum of the covariances of the squared loadings between all possible pairs of factors be minimized, Kaiser (111, 116) outlined the generalization of his varimax criterion to the oblique case.

Probably the best intuitive explanation of the notion of simple structure yet to appear was given by Tucker (200) in an introduction to his most recent semianalytic procedure. An interesting variant of the simple structure theme was presented by Schmid and Leiman (173) who proposed a "hierarchical ordering of factors." Altho a typical solution contains several more factors than the traditional Thurstonian approaches that make use of simple structure, it reveals simultaneously both simple structure and bifactor patterns while maintaining orthogonal axes.

Factorial Invariance

To the psychologist perhaps the final test of the efficacy of factor analysis for the scientific study of the internal structuring of correlated variables is the notion of factorial invariance. Ahmavaara (4) published a definitive monograph on the effect of selection. He provided methods of computing transformation matrices aimed at the problem of comparing different factor studies and applied his procedures in some detail to Thurstone's classic studies of primary mental abilities. In his monograph Rasch (156) also considered the stability of factor loadings under changes in population. Another methodological paper on invariance was that of Barlow and Burt (12), who contended that two factors may be viewed as identical if, when the persons are the same, the two sets of factor-scores are identical, or if, when the tests are the same, the two sets of factor loadings are identical.

Extending their earlier notion of "parallel proportional profiles" to the general case, Cattell and Cattell (31) were effectively concerned with finding the unique transformation matrix for rotational purposes on the basis of two experimentally independent studies of the same variables relative

to which the effect of selection is systematically varied. Another noteworthy paper was that of Wrigley and Neuhaus (214) who considered the matching of two sets of factors.

Inverse Factor Analysis

Interest in the problems of inverse factor analysis continued. In considering conditions under which Q and R technics are convertible, Block (20) concluded that homogeneity of interaction between variables should exist for all individuals, and in another paper Block (19) argued in favor of forced rather than unforced Q-sorting procedure. Also comparing R and Q technics in the factor analysis of four groups of geometrical solids, Lorr, Jenkins, and Medland (126) found that a two-way factor analysis may be meaningful if not preferable to reliance on one method. How Q-sorts may be used as a rating technic in various educational and personnel settings was discussed in an expository paper by Morsh (137).

Computational Procedures

Computational savings in factoring relative to an existing simple structure hypothesis were described in two papers by Horst (99, 100) and in one by Horst and Schaie (101). Rodgers (158) devised a fast approximate algebraic rotational procedure, and in inverse factor analysis Bass (14) reported a quick iterative means for clustering persons.

Among other noteworthy developments in factor theory were two papers by Guttman (81, 84), in the first of which he described a generalized simplex for factor analysis and in the second of which he developed a simplified formula for matrix factoring. In a brief note Durand (55) explained and illustrated the inversion of a matrix by a square root method.

Miscellaneous Developments

The determination of factor scores for individuals was the subject of a long and comprehensive paper by Guttman (80) who made a definitive examination of the problem. His interesting, tho not encouraging, findings revealed that for the same observed data alternative sets of such scores are usually available that exhibit little relationship to one another.

Finally mention should be made of the important five-day international colloquium in factor analysis held in Paris during July 1955, under the auspices of the *Centre National de la Recherche Scientifique* and the Rockefeller Foundation. The roster of 59 participants was internationally representative of different theoretical viewpoints and of such diverse fields of application as psychology, education, sociology, biology, and medicine.

Published in a book entitled *L'Analyse Factorielle et Ses Applications* (42), the proceedings include 23 main papers, discussions, and resumés. Containing a singularly complete coverage of virtually the entire discipline of factor analysis, the volume affords critical comparisons of technics and rationales that would be of interest both to the unsophisticated and to the mature student in the field.

The theoretical papers, which comprise about half the volume, offer an intensive and extensive treatment of current variations in conceptual and methodological approaches to factor analysis. Several of the theoretical papers include excellent historical reviews concerning the development of factor analytic technics that serve to delineate in detail the empirical bases for the divergent practices among the British, the Continental, and the American groups. In short, the individual who is interested in acquiring an overview of the current research being done in factor analysis will probably find this volume to be the best single source.

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CHAPTER IX

Data Processing: Automation in Calculation

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A WIDE variety of computational aids is currently available to educators, ranging from slide rule and desk calculator to punched-card equipment and electronic computation. In this review particular attention will be given to electronic computation, for several reasons: (a) The electronic computer will probably be the preferred aid in extensive calculations whenever available; (b) the machines require the more radical changes in research methods; and (c) the area is less known. But since much large-scale educational calculating is currently done with punched-card equipment, developments in that field will thereafter be briefly summarized.

Much of the computational literature deals with topics, such as numerical analysis, computer logic, computer engineering, scientific applications, and business applications, which possess no specific reference to education or psychology. Instead of trying to make a comprehensive survey of these fields, the policy will be followed of supplying certain key references, generally themselves well documented and of recent date, which provide starting points for any reader wishing to explore further.

Sections of the review are bibliographies, journals and professional societies, types of computers, digital computers, computer availability, programing an electronic computer, numerical analysis, computer use in education and psychology, factor analysis, punched-card procedures, other computational aids, and the brain-machine analogy.

Bibliographies

Devoe (32) prepared an unpublished bibliography on the use of computers in psychology (45 titles). Abstracts of the general computer literature appear in *Mathematical Abstracts, Mathematical Tables and Other Aids to Computation,* and *IRE Transactions on Electronic Computers.* The latter journal has an annual review of computer progress which includes extensive references (58, 81). The most comprehensive bibliography is the IBM one on use of punched-card machines in science, statistics, and education. The latest edition, compiled by Alsop, Flanagan, and Hankam (3) in 1956, has 762 references. These pertain chiefly to conventional punched-card equipment, but there is a scattering of references relating to electronic computers. Casey and Perry (21) included a 276-item bibliography in their discussion of punched-card methods in science and industry.

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Professional Societies, Journals, and Reference Works

The first journal devoted specifically to computation was *Mathematical Tables and Other Aids to Computation*, first issued in 1943. The principal professional organization in the area is the Association for Computing Machinery, founded in 1947, which now has more than 2000 members.

Three more recent journals relating specifically to electronic computation are *Computers and Automation*, published since 1952; *IRE Transactions on Electronic Computers*, published since 1952; and the *Journal of the Association for Computing Machinery*, published since 1954. (The ACM issued *Proceedings* in 1952 and 1953.) The *IBM Technical Newsletter* is another useful source. "A Who's Who in the Computer Field" (27) and "A Computer Directory and Buyers' Guide" (26) have recently been prepared.

Types of Computing

Calculating machines may be divided into two classes, digital and analog. A digital machine operates directly with numbers, whereas an analog machine represents them by physical quantities, for example, intensity of electrical current or length of line. Thus an abacus is digital, whereas a slide rule is analogical. Educators have been concerned primarily with digital machines (altho the IBM test-scoring machine is analogical), and this emphasis will probably continue since analog computers, because they depend on measurement of physical quantities, are limited in accuracy. Hence only passing reference will be made here to the analog computer literature. Berkeley and Wainwright (12) compared digital and analog computers and discussed the advantages and disadvantages of each. An introduction to analog computer technics was prepared by Johnson (62) while Wadel and Wortham (109) listed the location of some of the principal analog computer installations.

Digital computers can be divided into those with internal storage of programs (the orders which control the machine during the calculations) and those which rely upon a control panel for instructions. For this review, the term *electronic computer* will be confined to machines with internal program storage. Machines which rely upon control panels will be classified as *punched-card equipment.* Internally stored programing enables greater computational flexibility and higher speed.

Digital Computers

A wide range of books and articles is available dealing with design, programing, and scientific application of digital computers. Development has been so rapid that recent titles provide the best start on a review of the literature. Surveys by Wilkes (113) and Booth and Booth (14) supply useful introductions. A special issue of the *Proceedings of the IRE* (57)

was devoted entirely to electronic computation and covered a large variety of topics. Richards (90) was concerned with the basis in Boolean algebra of computer arithmetic. Bowden's edited symposium (15), altho slightly dated, deserves mention on account of its third section dealing with computer applications since the scientific usefulness of the machines in a wide variety of fields is indicated. (There is, however, no specific mention of computer use in educational research.)

The original objective in computer design was to make feasible those calculations of such great length that they otherwise either could not be performed at all or only with prohibitive labor. More recently it has become clear that computers will be equally important in achieving more rapid processing of comparatively simple clerical tasks. Business applications of computers range from maintaining a stock inventory to controlling airline reservations. This experience is relevant to educators because the same characteristics of large amounts of data and relatively simple calculations sometimes also pertain to educational research and record-keeping. Developments in commerce and industry have been reviewed by Kozmetsky and Kircher (67) and by Hattery and Bush (54).

Computer Availability

When Fattu surveyed the computational field in 1951, he concluded: "In the none too distant future it is likely that a substantial group of universities and social science research organizations will have access to automatic computing equipment" (37: 425). This prediction has been substantiated in the ensuing six years. In 1951 there were few electronic computer installations, and those in operation were principally devoted to national defense problems. Since there had been no educational or psychological use when Fattu prepared his review, his references to electronic computation had to be drawn from the technical literature of engineering, physics, and applied mathematics. But the computer field was at that time on the threshold of rapid expansion, including commercial production of machines. By an interesting coincidence, it was in December 1951, the month when Fattu's review was published, that an electronic computer was first used in psychology (118). The machine was Ordvac, the first electronic computer built at the University of Illinois; it was made available for use by all University departments.

The 1957 picture is greatly different. Over 1200 computers are reported to be in operation in the United States, and there are many installations in other countries (18). Computational centers have been established at a number of universities and research organizations. Machine speeds have increased as newer computers have become available, so that the terms *high-* and *medium-speed* must be regarded as relative. Here the term *high-speed* will arbitrarily be taken to apply to machines which multiply at a rate of more than 1000 times per second. Expansion has been so rapid that it is difficult to secure a complete list of universities and research organizations

with high-speed computers. Goode's tabulation (41), altho only two years old, is already dated, and Weik's survey (112) does not list all installations. Hence it has been decided to include a listing here, even if it proves incomplete.* High-speed computers are installed at University of California, Berkeley (IBM 701); University of California, Los Angeles (IBM 705; SWAC); Cambridge (Edsac); Harvard (Univac); Illinois (Illiac); Institute for Advanced Study (IAS); Massachusetts Institute of Technology (IBM 704; Whirlwind); Michigan State (Mistic); Pennsylvania (Univac); RAND Corporation (IBM 704; Johnniac); Sydney (Silliac). IAS, Illiac, Mistic, and Silliac are machines built to the same basic design. Iowa State College is building another machine of the class.

Medium-size computer installations include: IBM 650's at Armour Research Foundation, Boston University, Carnegie Institute of Technology, Case Institute of Technology, Cornell, Educational Testing Service, Florida, Georgia Institute of Technology, Houston, Indiana, Iowa State College, Kansas, Kentucky, Michigan, New York, North Carolina State College, Ohio State, Oklahoma, Oklahoma A. and M., Pittsburgh, Rochester, Stanford, Texas A. and M., Washington (Seattle), Washington (St. Louis), Washington State College, Wayne, and Wisconsin; Datatrons at California Institute of Technology, Chicago, Dayton, and Purdue; Ferrantis at Manchester and Toronto; Univac Scientific 1103A at Johns Hopkins; Univac Scientific 1101 at Georgia Institute of Technology; Pennstac at Pennsylvania State; Udec at Wayne; Elecom at Stevens Institute of Technology; Alwac at the Adjutant-General's Office; and CRC's at the School of Aviation Medicine and the Naval Postgraduate School. It is not known how many of these installations are available for educational and psychological research.

The role of a university computational center was discussed in a symposium edited by Hammer (50). Consideration was given to the equipping and financing of a center; the decision whether to lease, buy, or build a machine; training of personnel; development of better numerical methods; organization of a numerical analysis and computational curriculum; and calculations characteristic of various departments. Alman (2) and Gotlieb (43) contributed other descriptions of university computational centers.

As a guide to machine selection, Weik's survey (112) gave speed of operation, size of storage, input-output mechanism, rental or purchase price, and the like, for 103 makes of digital computers; Carroll (19) compared some computers in commercial production; and Bauer (11) provided machine specifications for half a dozen of the best known high-speed machines. Thomson, Harper, and Sawyer (99), in an American Psychological Association symposium, discussed the experience of the Personnel Research Branch, Adjutant-General's Office, in acquiring a computer. Recommendations as to the augmentation of the supply of trained man-

* The author would appreciate being informed of any errors or omissions.

power (especially trained programers) were made by a Wayne University conference (61). Rowan (93) considered selection of programers by means of psychological tests.

Programing—Directing an Electronic Computer

Most people know by now that the principal difficulty in computer use is the writing of programs, that is, the sets of orders which control the machine during the calculations. McCracken (73) recently issued a book on digital computer programing. His concern was not with any particular machine, but with the general logic made applicable to a fictitious one. Interesting attempts were made by Ward (110) and Hamming (51) to express the general principles of programing in brief articles.

Manuals giving programing instructions and order codes are available for most types of machines. A program for matrix multiplication presented by Cattell (23) in an appendix to his factor analysis textbook illustrates the formal organization. (It is also of interest as apparently the first program specifically prepared for psychological and educational use.) The programs for Edsac, the Cambridge computer, were reproduced by Wilkes, Wheeler, and Gill (114), and these authors rightly stressed the importance for any installation of developing and recording a program library as rapidly as possible. Frank (39) listed currently available Univac programs, and Wrigley (116) did likewise for Illiac.

An outstanding need is for a complete list of IBM 650 programs of use in educational research. At present only partial lists seem to be available, for example, abstracts issued by the IBM Applied Science Customer Assistance Group (60) and libraries listed by the University of Washington (107) and the Datamatic Corporation (25). The IBM library includes programs for analysis of variance, chi square, correlation, multiple correlation, phi coefficient, autocorrelation, matrix multiplication, matrix inversion, latent rods and vectors, and roots of algebraic equations. The University of Washington has programs for basic statistics (means, standard deviation, *t*-test, and the like), correlation, multiple regression, principal axes factor analysis, grade prediction, predictor selection, and optimal test time. The University of Wisconsin has a program for maximum likelihood factor analysis (52). The quartimax method of rotation in factor analysis was programed at Michigan, and the biquartimax method at Harvard (20).

Andrew L. Comrey (University of California, Los Angeles) prepared and mimeographed various correlational and factor-analytic programs for SWAC; Jack Block (University of California, Berkeley) did likewise for the IBM 701; Kern W. Dickman (University of Illinois) for Illiac; James C. Lingoes (Michigan State University) for Mistic; and Jack O. Neuhaus (University of California, Berkeley) for the CRC 102-A.

"Automatic" programing systems designed to leave as much of the labor of program organization as possible to the machine are now available

for a number of computers. For example, Poley and Mitchell (87) prepared a manual with the interesting name of SOAP (an abbreviation for Symbolic Optimal Assembly Program) which describes a mechanized system of assembly for a 650 program.

Now that many organizations are using computers, better arrangements for program reporting and distribution are urgently needed. Programing is expensive: The cost has been estimated to average $2 per instruction, and the usual program has some hundreds of instructions (42). Hence cooperation is imperative if machines are to be used economically. Several groups have recently been formed to try to develop coordinated writing of and interchange of programs. A group known as SHARE (the Society to Help Avoid Repetitive Effort) is concerned with the IBM 704 (5); another group called USE (Univac Scientific Exchange) is fulfilling a similar function for the Univac Scientific 1103 A; and a Midwestern University Computer Users Committee is primarily concerned with the IBM 650.

Numerical Analysis

As might be expected, there has been a spate of books concerned with the theory and practice of numerical analysis. But methods continue to be discussed for the most part in terms of the desk calculator situation. From among the many methods available for most standard calculations, for example, solving algebraic equations, there is not yet consensus as to the most effective methods for electronic computations.

Dwyer's textbook (34) on linear computations probably remains the best single reference for the educator altho written with a desk calculator orientation. Rao (88) discussed various multivariate technics of potential importance to education, including some readily applied only upon an electronic computer. Textbooks covering the standard numerical course were written by Hildebrand (56) and Nielsen (83). Forsythe (38) and Luke (71) listed selected references in numerical analysis.

Computer Use in Education and Psychology

This section deals with the use of computers in educational and psychological research. Some associated developments in statistical method and research design are included. Since considerable attention has been given to factor analysis in order to restate it in mechanizable form, it will form the subject of a separate section.

Predictive studies are an obvious avenue for computer use. Previously the number of predictors has generally had to be limited to 10 or 12 on computational grounds. This restriction is removed by computers. Davison (29) used 34 predictors in her study of degree of frustration tolerance displayed by nursery-school children. These included several product terms enabling weight to be given in the regression equation to interactions ob-

served between predictors. There is, of course, a greatly enhanced problem of shrinkage as the number of predictors is increased, but cross validation provides some protection until such time as the statistician can supply a really satisfactory answer. Simon (97) employed multiple criteria in a predictive study of Air Force mechanics. Each criterion measured a different dimension of job performance; dimensions were selected by a factor analysis of the criteria. Merrill and Bennett (79) discussed the application of temporal correlation technics in psychology in terms of electronic computer potentialities. Leiman (69) described the distribution of nearly 12,000 airmen to jobs in accordance with differential aptitude indexes and predetermined quotas. Lee (68) considered computer use in nonlinear multivariate prediction, and Ziegler (121) developed a computer procedure for determining biserial correlations of items with a criterion.

Computers have been used for experimentation upon the arithmetical properties of sampling problems and probability chains. The principle of the Monte Carlo method (80) is to estimate a quantity by random sampling rather than approximate it by calculation. A computing program is devised to calculate a sequence of numbers satisfying statistical tests for randomness. Its greatest usefulness probably applies to cases when there are stochastic processes. Tocher (100) discussed the application of computers to sampling experiments which can be imitated in the computation. Rosenthal and Ferguson (91) used a pseudo-random sequence to determine the sampling distribution of the Friedman nonparametric test. Block (13), using empirical data (correlated items drawn from the Minnesota Multiphasic Personality Inventory), examined the proportion of items which indicated two samples to be significantly different at some given probability level when in fact the population had been randomly divided into two parts.

Two distinctly novel uses of computers were reported. Green (45) devised a procedure for preparing stimuli for form perception experiments with random elements introduced by generating displays on an oscilloscope by a computer program and photographing them on filmstrips. As a contribution to the rapid processing of, and decision making upon, a large amount of information (e.g., relative positions of many fast-moving aircraft), Rowan (92) used a computer, first to generate a simulated flight pattern, and then to synthesize the data and to make appropriate decisions on the best method for handling the situation.

Tucker (105) seems to have been the first to consider the research potentialities of computers. He illustrated his discussion by proposing use of a computer for selecting test items with respect to their difficulty in such a way as to maximize validity. The suggested function was of such complexity that use of an electronic computer would be mandatory. Wrigley (116) summarized advantages and disadvantages of using computers in psychological research and indicated some likely changes in numerical practice and research design.

Factor Analysis

Factor analysis has traditionally involved recourse to human judgment in a way quite foreign to any other standard psychometric technic. High-speed machines, however, have made it essential to rewrite factor analysis (or any other classificatory technic which seeks to supersede it) in completely objective form.

In a review of contemporary factor analysis, Cattell (24) devoted a section specifically to the effects of the new computational aids upon factor-analytic design. The need for more mechanizable procedures seems to have been largely responsible for the developments in communality estimation and in analytic methods of rotation already reviewed in Chapter VIII.

When a computer is available, some mathematically more defensible method than the centroid can be used. The choice seems to lie between the Pearson-Hotelling principal axes method and the Lawley-Rao maximum likelihood method. Wrigley and Neuhaus (119) described the use of a computer for calculating principal axes solutions. Harris and Peirce (52) described a maximum likelihood solution adapted to the IBM 650. Rao (89) developed the maximum likelihood method (which he preferred to call the canonical method of factor analysis) in an especially convenient form for electronic computation, thereby supplying factor analysis with a mathematically derived test for the significance of factors. The feasibility of the maximum likelihood method, now that computers are available, was illustrated by Lord (70) in his analysis of speed factors (a 39-variable study). Because of the double iteration, however, to determine both the communalities and the number of significant factors, the method is slow to converge unless initial estimates are reasonably accurate.

Comrey and Levonian (28) used a computer to compare the factors derived from the use of three different coefficients of correlation (the tetrachoric correlation, the phi coefficient, and the corrected phi coefficient, i.e., phi divided by maximum phi) and concluded that the phi coefficient was more acceptable for factor-analytic work than generally believed. Wrigley and Dickman (117) used an index of factorial matching to study sampling variability of loadings when a sample of subjects is randomly divided into two.

Better computational aids are making possible factor analysis of large sets of items. Osgood and Suci (85) used a computer in a factor analysis of adjectival scales to determine the dimensions of meaning. Comrey and Levonian (28) showed the practicability of determining the factorial structure of the items of as large an inventory as the MMPI by making centroid analysis of the principal scales in succession. Wrigley and others (120) factored a set of 200 dichotomous items measuring aircraft mechanics' performance by modifying the square root method to enable systematic selection of pivot variables. Cattell (22) discussed the possibility of enhanced use of P-technic because of computational developments.

The possibility of a completely objective and therefore completely mechanizable system of factor analysis was recently demonstrated by Wrigley (115). He proposed insertion of squared multiple correlations instead of communalities in the leading diagonal, analytic rotation of all factors with positive latent roots, dividing the sample randomly into two and repeating the procedure for each section, and then calculating indexes of matching for the two sets of factors to determine the replicability of each factor.

Canonical analysis generalizes the multiple correlational procedure to the situation where there are multiple dependent variables, and each criterion dimension is to be predicted separately. Computers make this technic (along with much other disused multivariate algebra) practicable. Healy (55) reported a rotational method for computing canonical correlations.

Other investigations have sought to replace factor analysis by some other technic taking more into account the patterns and clusters revealed by the data. Altho this configurational approach seems to accord with clinical experience in psychology, and also with holistic theoretical conceptions, the development of satisfactory statistical methods has been delayed by the computational labor of examining a multiplicity of patterns. McQuitty (77, 78) devised new technics for handling the configurational problem, named agreement analysis and linkage analysis respectively, which have been oriented toward use of an electronic computer. Likewise Tryon (104) modified his method of cluster analysis to make use of the new machines.

Punched-Card Procedures

Ten years ago a sharp distinction could be made between electronic computers and punched-card equipment. The latter included sorters, tabulators, reproducing punches, multiplying punches, and the like. The boundary is no longer so distinct. Electronic computers often have punched-card input and output, while multiplying punched-card units may have vacuum tubes for more rapid calculation. The dichotomy based upon whether or not programs are internally stored means that the IBM 604 and 607 are here classified as punched-card equipment. Their electronic components, however, might entitle them to be classified with the electronic computers.

General surveys of punched-card operations were prepared by Casey and Perry (21) and by Hartkemeier (53), while a manual was prepared outlining the fundamental principles, applicable to all types of punched-card machine, of control-panel wiring (59). Gruenberger (48) issued a computing manual based upon university use of punched-card equipment at the University of Wisconsin, and later supplied control-panel diagrams for some standard computations (49). Lunneborg, Wright, and Ax (72) published some plugboard diagrams prepared at the University of Washington.

Appel and Cooper (4), Bass and Wurster (10), and Deemer (30) considered the use of mark-sense cards to eliminate card-punching and card-verifying. Traxler (102, 103) discussed the merits and demerits of the test-scoring machine, and Staugas (98) dealt with test scoring when item data were already punched on the cards.

Correlational technics received a good deal of attention. Ayers and Stanley (8) described a rolling totals method for forming sums of squares and cross-products, while Burke (16) dealt with the special case when some numbers were negative. Procedures for solving multiple regression equations were presented by Allan and Attridge (1) and by Greenberger and Ward (46). The calculation of serial correlation coefficients was described by Payne and Staugas (86) and by Schipper and Gruenberger (94), and the computation of residual matrices in factor analysis was handled by Friedman and Ward (40).

Various papers related to item analysis and test construction. MacLean and Tait (76) dealt with the computation of item and test means, variances, correlations, and item-selection indexes. A method for calculation of a joint occurrence matrix was described by Grace (44), while Caffrey and Wheeler (17) devised a new chi-square formula which could be more simply handled on punched-card equipment. DuBois, Loevinger, and Smith (33) devised a method called edge punching for calculating variances and covariances of dichotomous items. Farrell and Stern (36) reported on calculation of tetrachoric correlations; and Siegel and Cureton (96), on the calculation of biserial correlations for the evaluation of items.

Kephart and Oliver (65) dealt with the scoring in the method of paired comparisons, and Kahn and Bodine (63) discussed Guttman scale analysis. The analysis of factorial experiments was considered by Bainbridge, Grant, and Radok (9), and the calculation of an uncertainty function in information theory by Newman (82). Katz (64) described the analysis of multiple-level sociometric data. Ward (111) illustrated the great saving in time made by an IBM 607 by reanalyzing data.

Other Computational Aids

Even in a period dominated by the emergence of the electronic computer, the simpler computational aids were not entirely neglected. Gruenberger (47) advised on selection of a desk calculator, and Dwyer (35) considered the circumstances in which a desk calculator is to be preferred to punched-card equipment or electronic computers. Arnold (6) wrote a book on use of the slide rule.

The Brain-Machine Analogy

While there is general agreement that an electronic computer is very different in its operation from a human brain, the points of comparison and contrast continue to interest scholars and led to a provocative and

challenging series of papers. A symposium of the Institute of Radio Engineers (101) provides a good start for any reader wanting to pursue this line of investigation. Other important papers were written by McCulloch (74), Turing (106), MacKay (75), Von Neumann (108), Wilkes (113), Ashby (7), and Shannon (95). Specific consideration was given to machine "learning" and machine "insight" by Oettinger (84) and Deutsch (31) respectively, while Kochen (66) considered "group behavior" of robots.

Summary

This review affords only a glimpse of the activity in the computational field from 1951 to 1957. Many references could not be cited for lack of space, and much other computational development remains unpublished. By now many universities have computing centers, and educators are regularly working with calculating machines and becoming familiar with their strengths and limitations. But the field is in a transitional state. There remains a shortage of trained personnel; there is redundant programing and rather little interchange of information from one organization to another; program libraries are often inadequately reported; there has probably been too heavy an emphasis on the traditional procedures of multivariate analysis; and the full range of mathematical and statistical potentialities has hardly yet been explored. Fattu's comment of 1951 remains appropriate to a considerable extent in 1957: "Adapting these new technics to research remains." But the foundations have been laid.

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CHAPTER X

Action Research: A Teaching or a Research Method?

BERNARD R. CORMAN

CLOSING his 1953 review of action research, (the terms *cooperative* and *cooperative action* are favored currently), Wann (30) remarked that an action methodology was only beginning to emerge and that additional experimentation would be needed to make it practical for teachers to carry on research of a high quality. Since this major survey of the literature, the REVIEW has carried a number of reports on the further development of the action-research line (11, 21, 29). It seems desirable, therefore, to evaluate these efforts as contributions to methodology as well as further to catalog the literature. As an organizing theme we are returning to Wiles's earlier queries (32) on a distinction between action research and inservice training.

If we examine the writing about action research, it becomes apparent that some difference of opinion exists, even among those who are most enthusiastic, as to whether the action research approach does, in fact, constitute a new research departure. There are those who contrast action research with traditional or fundamental research (2, 3, 4, 5, 15). In its sharpest form this separation gives rise to statements to the effect that, instead of borrowing without change research methods from the sciences, research workers in education are now striking out on their own and building new skills (4). In a more moderate vein the distinction is made on the basis of the kinds of problems researched (1, 5, 25), on the adaptability of the research findings to real situations (5), on the motivation of the researchers (7), on the kinds of generalizations sought (15), on the intrinsic value of the research to the practitioner (12, 17), and most often and most importantly on who does the research (3, 5, 25).

Do any of these distinctions, important as they are, provide the foundations for a new methodology? Our answer will depend, of course, on a definition. If we require, as a minimum condition, that a new methodology or technic give us a new way of organizing or analyzing phenomena so as to lead to the generation and test of new hypotheses or to fresh ways of testing old ones, it is questionable if any of the features central in the thinking of the action researchers make much of a contribution to research methodology (however great a contribution they do make to the inservice education of teachers).

This is a rather sweeping judgment, but it can be tested in several ways. If, for one thing, we examine the three major recent books concerned with action research (3, 14, 22), we will find that once the questions of getting research used and of getting teachers to hypothesize cooperatively about their concerns are dealt with, the business of testing these hypotheses must be faced. And at this point the discussions are reduced to the employment

of the traditional methodologies and technics. This reduction is discerned by the writers.

Others have seen the point as well. Thus Ahrens (1) spoke of the necessity of adapting the methods and procedures of the professional researchers. Shumsky (25) pointed out that the distinction between cooperative and individual research as separate processes was vastly overdrawn. Blum (5) saw the essential difference as one of the attitude of the researcher.

Finally, to support the conclusion above, we can examine the production of the action-research teams. These are some of the hypotheses that have resulted: that spelling will be improved if the material is chosen from "real" material rather than from spellers (1); that students will benefit if common experiences, alternative methods of word attack, and oral experiences precede reading (28); that a survey will identify the nature of the remedial reading problems in a school (17); that favored reading material depends on content rather than length (18).

The point is not at all that these hypotheses are trivial, for any method may be used to research trivia, and, moreover, there is no reason for questioning the authors when they state that the hypotheses were important to the teachers doing the research. The point is, rather, that these are hypotheses which could have been arrived at thru any of the traditional methods. The argument is even sharper when we examine the technics by which the hypotheses were tested. Checklists, case studies, tests of means, and similar devices were employed. As a contrast, compare the jump forward in the nature of the hypotheses which were made testable by the introduction of a technic like analysis of variance, for example (27).

What is at issue is not the purity of research. Rather it seems important to achieve clarity about the nature of action research in order to protect the positive contribution which the approach has made.

Common to many of the reported action researches are statements to the effect that teachers found cherished prejudices challenged, that leadership was developed, that lines of communication became clearer, that interest in research was engendered, and that curriculum change was facilitated (1, 13, 14, 16, 17, 18, 19, 24, 28). There are reports of success in incorporating the approach both in the training of teachers and in the teaching of public-school classes (10, 13, 20, 22, 26, 31). Any movement which will encourage a turn toward problem solving in teacher education needs to be nourished. This, it would appear, is the distinctive contribution which action research does make.

The emphasis on action research as a separate and distinctive modern research methodology to be contrasted with traditional methodologies seems self-defeating of this positive contribution. Such emphasis introduces conceptions which will not produce warranted evidence either about practical or impractical problems. One example is the conception that research findings become valued only at the point where the teacher replicates them (12). Strictly read, such a conception negates the possibility of learning anything from the research efforts of others. We would find our-

selves asking questions which have been asked and answered many times before, and each of us would be put in the position of accepting only the answers obtainable with our own technical skill (3). Stemming from this viewpoint is a revival of the conception that there is some fundamental difference between applied and pure research, which takes expression in repeated statements that action researchers are somehow not interested in generalization, but only in whether or not teachers are accomplishing the things that they hope to accomplish (4, 7). But as Prewett (23) suggested, if our goal is to channel teachers into research rather than research into teachers, we need more rather than less concern for theory.

The principal danger which results from a confusion of the teaching and research functions of action research is the justification for the relaxation of elementary safeguards of the warranty of evidence which results. It is one thing to defend oversimplified hypotheses (3), to overlook necessary controls (28), to encourage changing hypotheses in midstream (5), to ignore the problem of reliability in stating that teachers are best fitted to know what is going on in their classrooms (3), and so on, *if* our purpose is simply to get teachers to engage in group processes or to introduce them to problem-solving methods. It is something else again if we defend these practices as somehow contributing to the production of valid inferences.

The original point of departure of action research was the failure of educational research to play a significant role in changing practice (6, 8). The answer found was to change the research personnel, to involve classroom teachers more directly in the research. If this is to be achieved, it will mean that teachers will have to take the time and effort to acquire the necessary tools, and that rather than striking out on their own, more time will have to be given in the teacher-education program for the study of methods of inquiry. Corey (9) spelled out some of the implications of the action-research movement for the teacher-education program.

Finally, note has to be taken of Blum's argument (5) that the action-research movement is a revolt against the separation of fact and values. If educational research, whether done cooperatively or noncooperatively, by action or in action, by professional or amateur, is to be valued, it is necessary that the researchers make explicit the preferences that undergird their efforts, and tackle the problems which are most pressing rather than those which are most convenient.

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§ Membership begins January 1, 1958.

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Pierce, Truman M.
Sawin, Enoch I.
Sims, Verner M.
Swenson, Esther J.
Terry, Paul W.
Westover, Frederick L.

Arizona

Baker, Robert L.
Beck, Roland L.
Danielson, Paul J.
Heimann, Robert A.
Hunt, Jacob T.
Jelinek, James J.
Kelley, Victor H.
Ralston, Mack A.
Richardson, H. D.
Schutz, Richard E.
Wochner, Raymond E.

Arkansas

Bruce, Imon E.
Schmid, John

California

Abernathy, Ruth
Adams, Mrs. Georgia Sachs
Allen, William H.
Bell, Mrs. Ethel Tobin
Bell, Hugh M.
Bennett, Margaret E.
Blaha, M. Jay
Bowman, Howard A.
Bowman, Mrs. Lillie Lewin
Brookover, Wilbur B.
Brown, Edwin J.
Brown, Kenneth R.
Brownell, William A.
Brueckner, Leo J.
Bungé, A. Frederick
Buswell, G. T.
Cain, Leo F.
Carter, Harold D.
Clark, Edwin C.
Clark, Stephen C.
Clark, Willis W.
Coladarci, Arthur P.
Compton, John L.
Cosgrove, J. Clarke
Crawford, Bonner M.
Crum, Clyde E.
Dawson, Dan T.
Drury, Blanche Jessen
Durflinger, Glenn W.
Ebey, George W.
Elliott, Merle Hugh
Ellis, Betty Walters
Espenschade, Anna
Findlay, Donald C.
Freeman, Frank N.
Fritch, C. Lorene
Fry, Mrs. Vera S.
Gilbert, Luther C.
Gowen, John Curtis
Gray, Robert T.
Greene, Harry A.
Hall, George V.
Hanna, Paul R.
Hockett, John A.
Holmes, Darrell
Holy, T. C.
Hughes, W. Hardin
Hyde, Edith I.
Imhoff, Myrtle M.
Jacobs, Robert
Jantzen, J. Marc
Johnson, Leighton H.
Johnson, Loaz W.
Jones, Harold E.
Keislar, Evan R.
Knoell, Dorothy M.
Kyte, George C.
Lantz, Beatrice
Latchaw, Marjorie
Lefever, D. Welty
Leton, Donald A.
Lewerenz, Alfred S.
Lewis, Hazel M.
London, Jack
Lord, F. E.
Lucio, William H.
McClelland, James N.
MacConnell, James D.
McConnell, T. R.
McLaughlin, Katherine L.
Michael, William B.
Michaelis, John U.
Monroe, Walter S.
Moonan, William J.
Morgan, Walter E.
Morphet, Edgar L.
Naslund, Robert A.
Patterson, William Rex A.
Perry, Raymond C.
Petty, Walter T.
Polster, Arthur H.
Roemmich, Herman
Rugg, Earle U.
Russell, David H.
Ryans, David G.
Schunert, Jim R.
Seagoe, May V.
Sears, Jesse B.
Shay, Carleton B.
Sheats, Paul H.
Silberman, Harry F.
Smallenburg, Harry W.
Smith, John Allen
Sowards, Wesley G.
Stegeman, William H.
Stewart, Lawrence H.
Sutherland, Margaret
Taba, Hilda
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Thompson, Anton
Thompson, Helen M.
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Trione, Verdun
Tyler, Frederick T.
Tyler, Ralph W.
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Wandt, Edwin
Waterman, Ivan R.
Weeks, Harold L.
Weitzel, Henry I.
Williams, J. Harold

Colorado

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Anderson, Harold M.
Bailey, Helen K.
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Douglass, Harl R.
Feder, D. D.
Garrison, Lloyd A.
Grieder, Calvin
Grimes, Leslie K.
Jennings, Marion Jeanne
~~Kelly~~, Edward J.
Leake, James D.
Loomis, Arthur K.
Luker, Arno H.
Moore, Harold E.
Natale, Joseph Paul
Patterson, Gordon E.
Perdew, Philip W.
Ross, G. Robert
Shaw, Jack
Slaichert, William M.
Spears, William R.
Walters, Newell B.

Connecticut

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Burnham, Paul S.
Cutts, Norma E.
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Gerberich, J. Raymond
Goldberg, Arthur
Hellmann, Walter H.
Howell, John J.

Joll, Leonard W.
Karnes, John W., Jr.
Keck, Winston B.
McKone, Frederick W.
Martin, W. Howard
Nason, Doris E.
Nathanson, Jules L.
Northby, Arwood S.
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Sheehan, Wilfred J.
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Smith, Allan B.
Stoughton, Robert W.
Swann, Reginald L.
Thorne, Edmund H.
Trinkaus, William K.

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Clark, Zenas R.
DeLong, Arthur R.
Maw, Wallace H.
Parres, John G.
Ravitz, Leonard
Schwartz, Alfred
Stauffer, Russell G.

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Asher, John William
Barber, Joseph E.
Beach, Fred F.
Bradford, Leland P.
Burr, Samuel Engle, Jr.
Caliver, Ambrose
Carr, William G.
Carstater, Eugene D.
Conger, Louis H., Jr.
Conrad, Herbert S.
Coon, Beulah I.
Cooper, Shirley
D'Amico, Louis A.
Davis, Hazel
Dawson, Howard A.
Dreese, Mitchell
Featherston, E. Glenn
Fishback, Woodson W.
Flint, Helen M.
Fogel, Mrs. Marguerite S.
Foster, Richard R.
Frutchey, Fred P.
Goldthorpe, J. Harold
Greenleaf Walter J.
Haggerty, Helen R.
Hall, Roy Maxwell
Harrington, Wells
Hodgkins, George W.
Hornbostel, Victor O.
Hubbard, Frank W.
Hutchins, Clayton D.
Hypps, Irene C.
Isenberg, Robert M.
Jessen, Carl A.
Johnson, M. Clemens
Lambert, Sam M.
Lippeatt, Selma
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Matthews, Joseph L.
Maul, Ray C.
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Nelson, Kenneth G.
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Orr, David B.
Reason, Paul L.
Rein, William C.
Remmlein, Mrs. Madaline Kinter
Robinson, Glen E.
Scates, Alice Yeomans
Segel, David
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Buckingham, B. R.
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Walker, George H., Jr.

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Raymond, Dorothy

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Ostreicher, Leonard M.
Pindell, Watson F.
Stern, Bessie C.
Tatum, Beulah Benton
van Zwoll, James A.
Wiggin, Gladys A.
Woollatt, Lorne Hedley

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Banning, Evelyn Irene

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Burch, Robert L.
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Chase, W. Linwood
Cogan, Morris L.
Cornell, Ethel L.
Cotter, Katherine C.
Crossley, B. Alice
Crowley, Harry L.
Davis, John B., Jr.
Davis, Russell G.
Durrell, Donald D.
Eames, Thomas H.
Everett, J. Bernard
Gibson, R. Oliver
Gross, Neal
Gunn, Mary Agnella
Hayden, James R.
Herberg, Theodore
Hunt, Herold C.
Jones, Vernon
Kugris, Violet
Kvaraceus, William C.
Lincoln, Edward A.
McGauvran, Mary E.
Meder, Elsa M.
Moriarty, Mary J.
Moulton, John K.
Moynihan, The Rev. James F.
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Patterson, Franklin K.
Pitkin, Fred E.
Porter, Douglas
Read, John G.
Richter, Charles O.
Rulon, Phillip J.
Sargent, Cyril Garbutt
Savignano, Leonard J.
Schweiker, Robert F.
Stoke, Stuart M.
Sullivan, Helen Blair
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Walton, Arline J.
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Young, Marjorie A. C.

Michigan

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Anderson, Lester W.
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Bernstein, Allen L.
Blocksma, Douglas D.
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Corman, Bernard R.
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Frost, Maurice D.
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Hamilton, Jean F.
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Harrison, Harold J.
Harrison, James
Herrick, Theral T.
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Jensen, Gale E.
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Miles, Vaden W.
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Nemzek, Claude L.
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Pirie, Duncan A. S.
Potter, Muriel C.
Rankin, Paul T.
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Rugen, Mabel E.
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Saupe, Joe L.
Schwahn, Wilson E.
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Smith, Donald E. P.
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Winston, Ethna Beulah

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Flesher, William R.

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Cunningham, John G.
Davison, Hugh M.
Desing, Minerva F.
Detchen, Lily
Erdly, Calvin V.
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Flanagan, John C.
Ford, Fred C.
Garber, Lee O.
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Goodnick, Benjamin
Gordon, Hans C.
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Hatcher, Hazel M.
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Hilton, Thomas L.
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Maney, Ethel Swain
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Myers, Garry C.
Preston, Ralph C.
Shinaberry, Charles G.
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Stevens, Glenn Z.
Tate, Merle W.
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South Dakota

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Garrison, Martin B.
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Kent, Druzilla Crary
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McClurkin, W. D.
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Texas

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Bateman, Jessie W.
Burton, Floyd H.
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Daniel, Artie A.
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Flournoy, Frances
Freeland, Alma Moore
Fruchter, Benjamin
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Horn, Thomas D.
Jahnke, Henry A.
Judy, Chester Jack

Kress, Roy Alfred, Jr.
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McGuire, Carson
Manuel, Herschel T.
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Murphy, Alton C.
Murphy, Harold D.
Otto, Henry J.
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Sells, Saul B.
Shea, James T.
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Utah

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Virginia

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Hawaii

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Japan

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REVIEW OF EDUCATIONAL RESEARCH

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Forthcoming Issues

Philosophical and Social Framework of Education. February 1958. Herman G. Richey, *Chairman*.

Language Arts and Fine Arts. April 1958. Franklin H. Knower, *Chairman*.

Teacher Personnel. June 1958. Jacob O. Bach, *Chairman*.

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* Out of print.

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Forthcoming Issues

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LANGUAGE ARTS AND FINE ARTS. April 1958. Franklin H. Knower, *Chairman.*

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